Appendix A: Detailed Description of Alternative Hypotheses

"We use models to evaluate hypotheses in terms of their ability to explain existing data and predict other aspects of nature. We use models to combine what we know with our best guesses about what we do not know."

- R. Hilborn and M. Mangel, <u>The Ecological Detective – Confronting Models with Data</u>.

Table of Contents for Appendix A

A.1 Introduction

A.1.1 Structure and Purpose of This Appendix

The purpose of this Appendix is to provide more details about the alternative hypotheses outlined in Section 4 of the report. This Appendix is definitely a work in progress. Once completed, it will provide: 1) detailed biological rationale for alternative hypotheses and evidence against each hypothesis; 2) mathematical means of representing each hypothesis; and 3) ideas on how to resolve differences in future through research, monitoring or adaptive management actions. The first two pieces (biological rationale, evidence for/against and mathematical representation) are only partially complete. We have included comments on the rationale provided for alternative hypotheses, and in some cases, responses to these comments. These comments and responses are indented to separate them from the main text (responses are also italicized). Ideally, each key alternative hypothesis (i.e., those which have a significant effect on the decision) should account for the full set of available evidence. The assessment of alternative hypotheses against available evidence should be presented in a structured manner that facilitates scientific judgements on the appropriate assignment of weights. The third item (research, monitoring, and adaptive management approaches) has not yet been well developed, but will be in our final report.

The structure of the Appendix mirrors that of Section 4. Each sub-section in Section 4 (e.g., 4.2.3), which summarizes a set of alternative hypotheses, has a corresponding sub-section in Appendix A (e.g., A.2.3), which provides the above details for that set of hypotheses. As explained in Section 4.1, a particular combination of assumptions constitutes a prospective aggregate hypothesis, and several of these combinations could be consistent with one retrospective aggregate hypothesis.

A.1.2 Description of Modeling Approach

As we explained in Chapter 3, PATH modeling analyses of projected future effects of hydrosystem management actions are based on previous PATH analyses of historical data. In this section, we provide more details about the flow of information from the PATH retrospective analyses through retrospective and prospective modeling analyses to the performance measures of interest. Figure A.1-1 below, which is based on the simplified diagram shown earlier in Chapter 3 (Figure 3-1), lists and defines the parameters that are passed between the various models and analyses. References for more details on the parameters in each of the boxes are provided in Table A.1-1 below.

Table A.1-1:   Reference for more details about parameter estimates used in the models.

Box in Figure A.1-1.	Section in this report with further details
1	PATH 1996 Conclusion Document
	PATH 1996 Retrospective Report
2	A.2.3
4 and 11	A.3.2
5	Beamesderfer et al. 1997[1];
	Anderson and Hinrichsen 1997[2]
6 and 13	A.3.2
8	2
9	A.2.2; A.2.5; A.2.6
12	A.3.4, A.3.5, A.3.7

1   Spawner-Recruit Data for Spring and Summer Chinook Salmon Populations in Idaho, Oregon, and Washington. In: PATH Package #3 for the Scientific Review Panel. July 11, 1997.

2   Prospective analysis for the alpha model. In: PATH Package #4 for the Scientific Review Panel. August 5, 1997.
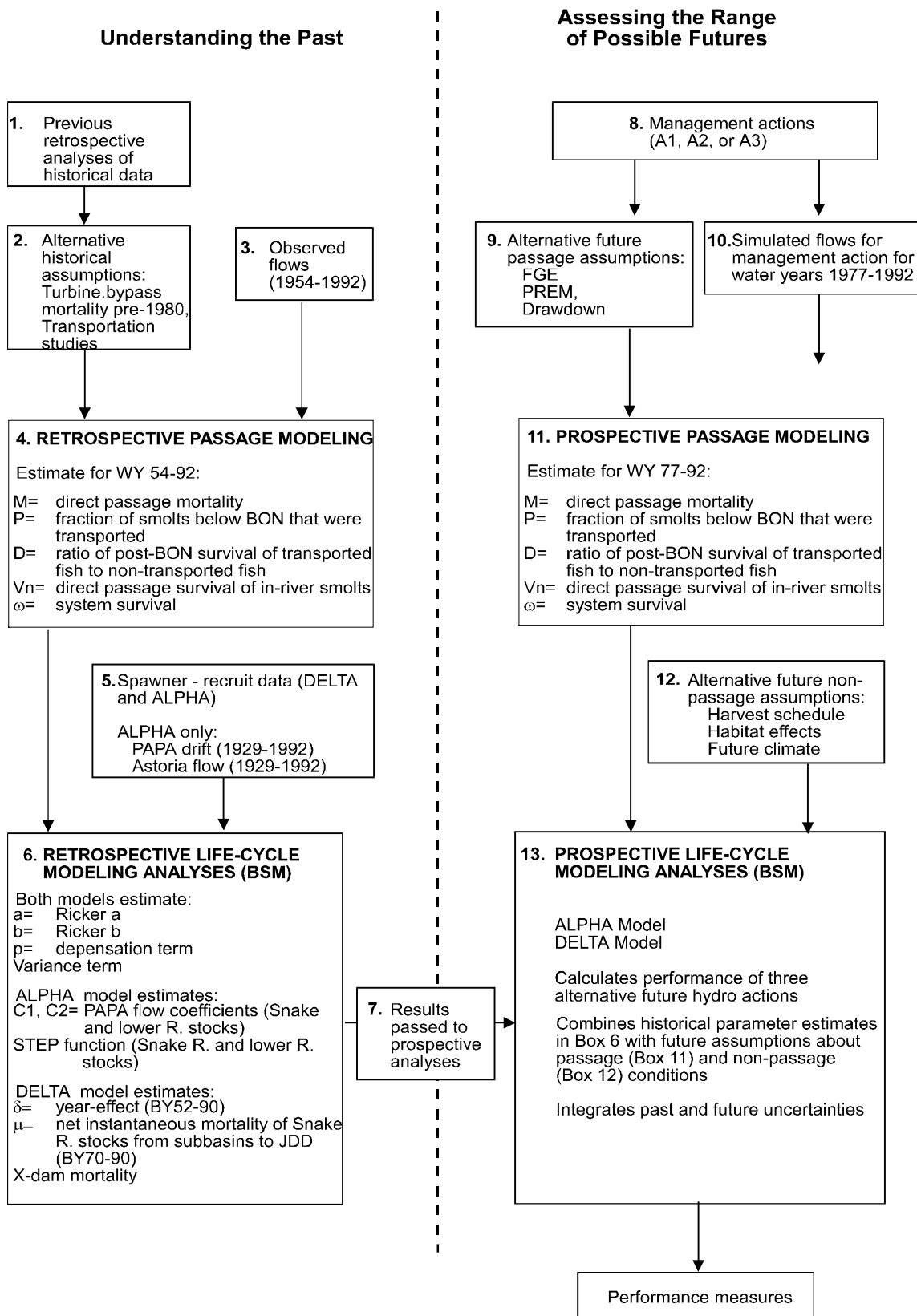
Understanding the Past

1. Previous retrospective analyses of historical data

2. Alternative historical assumptions: Turbine.bypass mortality pre-1980, Transportation studies

3. Observed flows (1954-1992)

4. RETROSPECTIVE PASSAGE MODELING

Estimate for WY 54-92:

M= direct passage mortality
P= fraction of smolts below BON that were transported
D= ratio of post-BON survival of transported fish to non-transported fish
Vn= direct passage survival of in-river smolts
ω= system survival

5. Spawner - recruit data (DELTA and ALPHA)

ALPHA only:
PAPA drift (1929-1992)
Astoria flow (1929-1992)

6. RETROSPECTIVE LIFE-CYCLE MODELING ANALYSES (BSM)

Both models estimate:
a= Ricker a
b= Ricker b
p= depensation term
Variance term

ALPHA model estimates:
C1, C2= PAPA flow coefficients (Snake and lower R. stocks)
STEP function (Snake R. and lower R. stocks)

DELTA model estimates:
δ= year-effect (BY52-90)
μ= net instantaneous mortality of Snake R. stocks from subbasins to JDD (BY70-90)
X-dam mortality

7. Results passed to prospective analyses

Assessing the Range of Possible Futures

8. Management actions (A1, A2, or A3)

9. Alternative future passage assumptions: FGE PREM, Drawdown

10. Simulated flows for management action for water years 1977-1992

11. PROSPECTIVE PASSAGE MODELING

Estimate for WY 77-92:

M= direct passage mortality
P= fraction of smolts below BON that were transported
D= ratio of post-BON survival of transported fish to non-transported fish
Vn= direct passage survival of in-river smolts
ω= system survival

12. Alternative future non-passage assumptions:
Harvest schedule
Habitat effects
Future climate

13. PROSPECTIVE LIFE-CYCLE MODELING ANALYSES (BSM)

ALPHA Model
DELTA Model

Calculates performance of three alternative future hydro actions

Combines historical parameter estimates in Box 6 with future assumptions about passage (Box 11) and non-passage (Box 12) conditions

Integrates past and future uncertainties

Performance measures

Figure A.1-1: Detailed diagram of analytical approach used in the decision analysis. See Table A.1-1 for sections providing further definition and explanation of parameters.

A.2 Uncertainties / Alternative Hypotheses Related to Downstream Passage

A.2.1 Passage Models

A.2.1.1 Structural Differences in Passage Models

The downstream section considered by the passage models is from the head of LGR pool to below BON dam. PATH has used two passage models in our analyses: CRiSP and FLUSH. The reason for using two models is that they represent different approaches to modeling reservoir mortality, dam passage mortality and transportation (barge) mortality. These are the three main components for which different hypotheses exist within these two models, and different ways of representing these hypotheses mathematically. Assumptions about the post-Bonneville effectiveness of transportation are also important (Section A.3.1); these assumptions are independent of the passage model used. In this analysis, particular passage models have been associated with specific transportation assumptions (i.e., FLUSH with TRANS1 or TRANS2; CRiSP with TRANS3).

The major uncertainties related to hydro actions involve the effects of the hydropower system on juveniles both during their downstream migration (including the effects of both passage through and transportation around the hydropower system) and any residual effects in the ocean. There is a hierarchy of three levels of hydro-related mortality processes as modeled by the two passage models, CRiSP and FLUSH (Figure 1 and Table A.2.1-1). Note that there may be uncertainties not only in the current values or structure of these components, but also in how these components are expected to respond to the hydro actions under consideration.

Level 1: At the most general level (Level 1) are the three major components of passage mortality in CRiSP and FLUSH: reservoir mortality, dam passage mortality, and transportation mortality.

Level 2: Level 1 components are composed of various sub-components of mortality. For example, reservoir survival is a function of flows, WTT, FTT, predation mortality, cumulative effects of stress, bioenergenic loss, injury, disease, and gas-related mortality; dam passage mortality is made up of turbine, spillway, and bypass mortality. These sub-components form the second level of hydro-related mortality. Because Level 2 mortality processes are modeled differently by the two passage models, uncertainties in these processes will also be different.

Level 3: The third level is represented by the precise nature of the functional relationships and equations that are used in the models to represent Level 2 processes of hydro-related mortality. For example, FLUSH uses a relationship between FTT and survival to model reservoir survival rates, while CRiSP uses a series of equations describing gas-related and predation mortality processes. The result is that the survival-FTT relationships are quite different between the two models (Figure 4.2-3).

Uncertainties associated with these Level 3 processes arise from three sources:

1. Uncertainties in the overall form of the relationship (e.g., is the relationship linear, exponential, logistic, etc.)

    These uncertainties should be considered explicitly where the main source of uncertainty is in the overall shape of functional relationship rather than in their exact parameterization, or where the effect of a management action is expected to alter the general form of relationships. Lack of data with sufficient contrast (e.g., insufficient transportation and reach survival studies at very long water and

fish transit times) can result in this type of uncertainty. This type of uncertainty is also present when several functional relationships are linked to from a relationship of interest (e.g., a predation function and a gas mortality function combine to generate a reservoir survival-FTT relationship).

2.   Uncertainties in the parameters that determine the shape of a given functional form.

These uncertainties should be considered where the functional form of a particular relationship is generally accepted, but the exact shape of that relationship as described by its parameters or the underlying data set is uncertain, or where the management actions are expected to affect one or more individual parameters in a relationship. Note that uncertainties in the functional form of relationships can be represented by changes in equation parameters to the extent that the relationship is flexible enough to represent the full range of uncertainty in its shape (e.g., changes in the reservoir mortality vs. FTT relationship in FLUSH).

3.   Uncertainties about which data sets or points should be used as the basis for estimating/calibrating functional relationships and their parameters.

These uncertainties should be considered where there are multiple data sets that could be used to fit a relationship (e.g., reach survival studies), or where there is disagreement over particular years of data because of the experimental design, the exact methods used to collect the data, or the potential effects of confounding factors (e.g., low/high flow years, change in hydropower system operations or configuration). The TURB and TRANS alternative hypotheses are good examples.

In general, uncertainties in the decision analysis should be as specific as possible (i.e., Level 3) because (a) alternative hypotheses at this level of detail are easier to represent quantitatively; and (b) it will allow the effects of these uncertainties on the performance measures to be more precisely understood. Obviously, explicitly considering every uncertainty would lead to an overly complex analysis, and we already have over 5000 aggregate hypotheses. Consequently, it is important to only incorporate those uncertainties that are the most critical in determining the model outputs that affect the ranking of management actions. The sensitivity of the decision to other uncertainties that are not incorporated into the decision tree can be dealt with through sensitivity analyses (carefully scoped to avoid prolonged and unnecessary analyses). Note also that each Level 1 component of hydro-related mortality does not necessarily have to be represented in the decision analysis, and that there may be more than one Level 2 and 3 mortality process represented in the tree for a given Level 1 process. The point to emphasize is that only those uncertainties that have the most effect on the performance measures should be represented in the decision tree.

To focus the discussion of uncertainties in the models, we have listed in Table A.2.1-1 below some of the Level 2 and 3 mortality processes associated with each of the Level 1 processes (i.e., reservoir, dam passage, and transportation mortality). Parameter names are shown in *italics* and are based on descriptions of CRiSP 1.5 (CRiSP Manual dated March 20, 1996) and FLUSH 4.6 (Spring FLUSH Version 4.6, Draft Documentation. August 25, 1995). Page numbers in parentheses refer to these documents, except where otherwise noted. This list is not exhaustive but instead attempts to identify the most important processes in each model that corresponds to the different levels of uncertainty. We also refer to Chapter 6 of the PATH FY96 Retrospective Analyses.

Table A.2.1-1: Hierarchy of components of hydro-related mortality

Level 1 Component	Level 2 Component		Representation of Level 3 Component in CRiSP	Representation of Level 3 component in FLUSH
Reservoir Mortality	Flow		flows provided by Hydro reg. modeling group	
	WTT		models to try to use similar WTT	
	FTT		models to try to use similar FTT	
	Survival vs. FTT implicitly includes: gas mortality, predation, cumulative effects of migration delay (e.g., energy depletion, temperature effects, osmoregulation)	Functional form	not directly applicable to CRiSP	Survival vs. FTT relationships for four different TURB assumptions
		Parameters		A, B (Ch. 6, App. 5)
		Data	Consistent set of FTTs and survivals used by both models (Toole et al. 1997)	NMFS Reach Survival studies, 1970-1980 (excl. 1971-72) 1994-1996 NMFS PIT-tag data
	Gas mortality	Functional form	Gas mortality function	not directly applicable to FLUSH
		Parameters	a, N_S, N_C, b, H (p. 86)	
		Data	Dawley et al. (1976)	
	Predation	Functional form	Predation mortality function:	Effects of predator removal program implemented through adjustment to *ResSurv* (p. 14)
		Parameters	$a, P(E), u,$ (p. 66)	
		Data used to parameterize/ calibrate	JDA Predation studies, 1984-1986; Predator Index studies, 1990-1993 ; Rieman & Beamesderfer 1990; Predator consumption study (Vigg et al. 1991)	
Dam Passage Mortality	Turbine mortality	Functional form	Turbine survival function	Turbine survival function
		Parameters	$p, y, S, F, m_{fo}, m_{tu}, fge$ (p. 135)	*Spillef, FGE, TurbSurv* (p. 15)
		Data used to parameterize/ calibrate	PATH Hydro Work Group Report (Oct. 1997)	

Level 1 Component	Level 2 Component		Representation of Level 3 Component in CRiSP	Representation of Level 3 component in FLUSH
Dam Passage Mortality (cont'd)	Turbine mortality (cont'd)	Other data sets available	Turbine survival (Ch. 6): PIT-tag estimates 1993, 1995; Balloon-tag estimates 1994-95; pre-1993 turbine survival studies (reviewed by Iwamoto and Williams 1993)	
	Spillway mortality[1]	Functional form	Spillway survival function	Spillway survival function
		Parameters	p, y, S, F, m_{sp} (p. 137)	*Spillsurv, Spillef* (p. 14)
		Data		
	Bypass mortality[1]	Functional form	Bypass survival function	Bypass survival function
		Parameters	p, y, S, F, m_{by}, fge (p. 136)	*Spillef, FGE, Bypasssur* (p. 15)
		Data		
	Spill Efficiency	Functional form	y = Seven possible equations	Currently assumed 1:1 at all projects except Dalles (function of spill proportion at Dalles). Other assumptions to be explored in sensitivity analyses (see Section A.2.4)
		Parameters	a, b (p. 119)	
		Data used to parameterize	Hydro acoustical studies	
		Other data avail.	Ch. 6 Appendix 4: Radio telemetry, experimental digitally-coded tags	
	FGE	Data used to parameterize	Krasnow et al., 1997	
		Other data avail.	Ch. 6: Fyke net estimates, 1989-93	
Transport. Mortality	Barge mortality[2]	Functional form	Constant	
		Parameters	p, y, S, F, m_{fo}, m_{by}, fge, m_{tr} (p. 136)	
		Data avail.	No direct measurements of barge mortality	
	Total mortality		See discussion under Section A.3.1 below	
	Proportion transported	Function of flows, FTOT rules		

1. Chapter 6 of FY96 Retrospective Report suggests that current values of these mortality rates are known with relative certainty (2%).
2. Barge mortality is assumed to be low (2-4%, Chapter 6 of FY96 Retrospective Analyses), but there are few good studies.

Alternative Hypotheses for Reservoir Survival

Assumptions about the processes driving reservoir survival are a major source of differences between the two passage models. Both models have used a common set of reach survival data for comparing their predictions to empirical information (Table A.2.1-2). Alternative hypotheses for reservoir survival can be represented as different values of relevant parameters in the passage models. For FLUSH, these parameters are *A* and *B* in the reservoir survival: FTT relationship; relevant parameters in CRiSP include the predator activity coefficient *a*, and the predator density *p(E)* (Table A.2.1-1).

Alternative Hypotheses for Post-BONN Survival of Transported Fish

CRiSP and FLUSH estimates of passage survival are each associated with different assumptions about the post-Bonneville relative survival of transported and non-transported fish. These assumptions are documented in detail in Section A.3.1, but the main differences can be summarized as:

1. Differences in retrospective values for V_n (direct passage survival of smolts passing in-river);

2. Differences in the set of years for T/C information used to estimate future T/C's and corresponding *D*'s (i.e., either 1971-1989 (FLUSH), or just the post-1980 years with transportation studies (CRiSP); and

3. Differences in the smoothing procedure used (i.e., smoothing (T/C) estimates (FLUSH) or smoothing (averaging or regressing) the *D* estimates (CRiSP).

Differences in V_n are caused primarily by differences in the relationships used to relate fish transit time to fish survival.

A.2.1.2   Detailed Diagnostics to Examine Passage Model Differences in Retrospective Simulations

The FLUSH and CRiSP models contain a series of linked hypotheses regarding direct survival from the head of Lower Granite pool to below Bonneville Dam. In this section, the critical hypotheses are examined first from the perspective of dam passage and second from the perspective of reservoir passage. Note that in this section, Ayear@ refers to the juvenile migration year, which is two years later than the brood year. These comparisons are performed for the historical or retrospective runs of the models, for migration years 1970 to 1997, which we call Scenario A0. The detailed outputs presented below provide examples of differences in behavior of the two passage models, when simulating the 1970 to 1997 period. This is the most comprehensive set of diagnostics produced to date for these two models. Considerable effort has been expended to maximize the consistency of the assumptions made by the two models for retrospective runs. Though this has resulted in a great deal of convergence in the retrospective simulations, some significant differences do remain. We are still exploring the reasons for these differences. *All of the figures referred to below are grouped together at the end of this section. Readers wishing to review critiques of each model should proceed to the text which follows these figures.*

Dam Passage Routing and Survival

Approach To evaluate CRiSP and FLUSH estimates of dam passage mortality, all reservoir survival functions in each model were set to a constant 100% survival. Therefore, the only mortality in these diagnostic runs resulted from dam passage.

Summary Estimates of total dam passage survival ranged from 10 - 77%, varying by year, TURB assumption (for years prior to 1980), and passage model (Figures A.2.1-1a-c, A.2.1-2a-c, A.2.1-3).

Annual Variation: On average, dam passage survival estimates were lower and more variable prior to 1980 than in more recent years. For TURB4 and TURB5 assumptions, 1973 and 1977 survival estimates were much lower than estimates in other years.

TURB Assumption Effects (Pre-1980):   Highest dam passage survival estimates with least variation resulted from TURB1 assumptions; the opposite situation resulted from TURB4 assumptions; and TURB5 assumptions resulted in intermediate estimates.

Passage Model Assumptions: Both FLUSH and CRiSP models estimated nearly identical temporal patterns, but the magnitude of survival estimates differed markedly (up to 14% absolute difference) among the two models for some years and some TURB assumptions. For 1980-**1997[?]**, FLUSH estimates were consistently lower than CRiSP estimates. Prior to 1980, relative performance of each model varied by year and TURB assumption. Differences among passage models were not expected and these differences do not represent specific alternative hypotheses. Perhaps the best way to think of the differences in dam passage survival among CRiSP and FLUSH models is as estimation error.

Detailed Results: 1970-1979 We examined three assumptions regarding bypass and turbine survival prior to 1980 (TURB 1,4, and 5) (see Section 4.2.3 for an explanation of TURB assumptions). We expected dam passage survival estimates to differ by TURB assumption but, for a given TURB assumption, to be nearly identical in the FLUSH and CRiSP models. This did not occur. Dam passage survival estimates through all projects ranged from 10-72% (approximately 75-95% mean per-project survival) and varied by year, TURB assumption, and model (Figures A.2.1-1a-c, A.2.1-2a-c, A.2.1-3).

Dam passage survival estimates were generally highest for TURB1 (46-72%, average 59% FLUSH and 60% CRiSP). Both models estimated the highest survival in 1971 and lowest in 1977 and generally paralleled each other in relative ranking of years. CRiSP estimates were <1 to 6% higher than FLUSH estimates in 1970-72 and 1977-80, while FLUSH estimates were <1 to 5% higher than CRiSP estimates in 1973-76. Disparity among models ≥5% occurred in 1970, 1974, and 1975. Possible reasons for the greater disparity among models in these years are being explored. Years with more daily variation in spill should have the largest differences between CRiSP and FLUSH, since CRiSP operates on a daily time step and FLUSH on a seasonal time step.

Dam passage survival estimates were generally lowest for TURB4 (10-72%, average 44% FLUSH and 43% CRiSP). Both models estimated the highest survival in 1971 and lowest in 1977 and generally paralleled each other in relative ranking of years. CRiSP estimates were 4-10% higher than FLUSH estimates in 1970-72 and FLUSH estimates were 2-14% higher than CRiSP estimates in 1973-76 and 1978. Estimates were nearly identical in 1977 and 1979. Disparity among models ≥5% occurred in all years except 1971 and 1977-79. Possible reasons for the great disparity among models in these years (and with this TURB option) are being explored; the most likely explanation is that the TURB4 rules were either implemented differently, or have a greater impact due to time step differences between the two models.

 TURB5 dam passage survival estimates were generally intermediate to those of the other turbine and bypass assumptions (36-72%, average 55% FLUSH and 62% CRiSP). As with other TURB assumptions, the relative ranking of years was generally parallel in each model. Unlike the other TURB assumptions, CRiSP estimates were consistently 1-14% higher than FLUSH estimates in every year. Disparity among models ≥5% occurred in all years except 1971 and 1975-77.

Detailed Results: 1980-1992   Specific hypotheses were not proposed for post-1979 dam passage, so estimates did not vary by TURB assumption and nearly identical FLUSH and CRiSP estimates were expected. Passage model diagnostics indicate that 1980-1992 dam passage routing and survival in the two models were similar, but not identical. Dam passage survival estimates ranged from 50-66%, with

survival highest in the more recent years (Figures A.2.1-1a-c). Mean per-project survival ranged from approximately 92-94% (Figures A.2.1-2a-c). CRiSP estimates were higher than FLUSH estimates by <1 to 6.4% (mean difference 4%) in all years (Figure A.2.1-3). Disparity among models ≥5% occurred in 1988 and 1990-92.

Detailed Results: 1993-1997   These juvenile migration years were not included in the prospective analysis because adult returns and run reconstructions are not yet complete. However, this information is useful for evaluating model performance We implemented two FGE hypotheses (FGE1 and FGE2), which are related to assumptions about guidance of extended-length screens at three projects in 1996 and/or 1997 (Section 4.2.2).

The 1993-1997 estimates were generally higher than estimates from earlier years, ranging from 63-77%. As with the 1980-1992 estimates, FLUSH 1993-1997 estimates were 6-11% lower than CRiSP estimates each year. The greatest discrepancy was associated with 1994. The FGE assumptions affect the proportion of fish transported, which combines with transportation assumptions (TRANS1, TRANS2, and TRANS3) to affect prospective runs.

Reservoir Passage Survival

To evaluate CRiSP and FLUSH estimates of reservoir mortality, all dam passage survival functions in each model were set to a constant 100% survival. Therefore, the only mortality in these diagnostic runs resulted from reservoir passage. In this section, CRiSP and FLUSH model performance is compared. The underlying functions that contribute to reservoir survival were examined in turn and an attempt was made to document the key hypotheses responsible for different model behavior.

Water and Fish Travel Time

FLUSH and CRiSP water travel time (WTT) estimates closely paralleled each other over time, but CRiSP estimates were consistently slower than FLUSH estimates (Figures A.2.1-4 and A.2.1-5). Discrepancies ranged from 0.4 to 5.4 days, averaging approximately 3.6 days. The greatest discrepancies (>4.9 days) were associated with 1970, 1972, 1984, and 1991 estimates.

The relationship between water travel time and fish travel time (FTT) appeared to be similar in FLUSH and CRiSP (Figure A.2.1-6). While each model uses a fairly complex set of reach-specific equations, the relationships for the full hydropower system can be approximated with the following linear regressions:

CRiSP: $FTT = 9.452 + 0.894*WTT$ $r^2 = 0.971$

FLUSH: $FTT = 4.039 + 1.099*WTT$ $r^2 = 0.986$

While the slopes are similar, the intercepts differ by over five days. On a yearly basis, travel time predictions of the two models were nearly identical for the two longest WTT years (1973 and 1977), but FLUSH estimates were 1.8-6.8 days faster than CRiSP estimates (average 4.4 days) in all other years (Figure A.2.1-7). The greatest discrepancies (>5.9 days) were associated with 1970, 1972, 1983-86, and 1991 estimates.

In summary, the greatest difference in travel time among the two models (average 3.6 days) appears to be associated with estimation of water travel time. However, an additional discrepancy associated with conversion of WTT to FTT increases the average difference in estimates to 4.4 days.

Reservoir Survival In Relation to Fish Travel Time

Two competing hypotheses are embodied in the FLUSH and CRiSP models: FLUSH estimates reservoir survival directly from fish travel time while CRiSP estimates it indirectly from exposure time to predation and total dissolved gas levels.

In FLUSH, the relation between fish travel time and reservoir survival varied with each TURB function because the TURB assumptions affect the partitioning of reservoir and dam mortality in the reach survival estimates to which FLUSH is calibrated (Figure A.2.1-8). The reservoir survival functions estimated with TURB1 and TURB5 were very similar, with a fairly steep exponential decline in survival associated with increased travel time over the range of estimated travel times. In contrast, the TURB4 survival function declined less steeply with travel time and generally resulted in higher survival estimates at a given travel time. When displayed as survival per day, all three FLUSH relationships showed a decreased reservoir survival rate as cumulative travel time through the system increased (Figure A.2.1-9). In other words, in the FLUSH reservoir survival functions, both the cumulative mortality and the daily mortality rate increased with cumulative exposure to the hydropower system.

In CRiSP, reservoir survival did not vary with TURB assumption because the predation and gas mortality functions do not use reach survival estimates in their calibrations. A clear relation between travel time and reservoir survival is not obvious in CRiSP because of the confounding effects of predation and gas-related mortality (Figure A.2.1-8). In some of the earlier years, large amounts of spill occurred with no mitigation from spill deflectors, presumably causing significant gas-related mortality in CRiSP, and the large variability in reservoir survival for a given FTT. We feel fairly confident in ascribing this cause to the results, because the CRiSP reservoir survival vs. FTT function showed very little variability in *prospective* simulations under management action A2, which has little spill (unpublished results, to be included in our final report). We should however confirm this by alternately disabling predation or gas-related mortality functions in CRiSP in the retrospective simulations (Figure A.2.1-10 [Note: CRiSP data not received yet]). Reservoir survival per day remains constant in CRiSP provided that there is no change in temperature. Since the predation rate is temperature-dependent, mortality tends to increase over the spring migration season as the river warms up (Figure A.2.1-11, not received yet).

CRiSP reservoir survival estimates were generally lower than FLUSH estimates between 1970-1976 and were generally higher than FLUSH estimates from 1977 to 1996 (Figures A.2.1-12 and A.2.1-13). Exceptions were a higher CRiSP estimate in 1973, higher FLUSH TURB4 and TURB5 estimates in 1974-76 and 1982-83, and higher FLUSH TURB4 estimates in 1994-95. Discrepancies among models and TURB assumptions ranged from <1% to 53%. At least one FLUSH TURBx estimate varied by more than 5% from CRiSP estimates in all years except 1982. On average, CRiSP estimates were 6.8% higher than FLUSH TURB1 and 3.9% higher than FLUSH TURB5 estimates, while FLUSH TURB4 estimates were 2.8% higher than CRiSP estimates. The greatest discrepancy was associated with 1971: FLUSH estimated 60-66% reservoir survival while CRiSP estimated 13% reservoir survival.

Upstream vs. Downstream Reach Survival (Dam + Reservoir Passage)

If the main difference between the CRiSP and FLUSH reservoir survival vs. FTT relationships is the assumption of an increasing mortality rate with time spent in the system in FLUSH (and the corresponding alternative assumption of a temperature-dependent but more constant rate in CRiSP), then differences in reach survival estimates between the two models should be greater in the lower river reaches than in the upper reaches. To evaluate model differences among upriver and downriver reaches, reservoir survival was estimated from Lower Granite (LGR) reservoir through John Day (JDA) reservoir and from John Day dam through Bonneville (BON) dam. The influence of these reaches on overall FLUSH vs. CRiSP in-river survival estimates was inferred from an exercise in which each model=s LGR-JDA estimate was paired with the other model=s JDA-BON estimate.

Survival estimates for the JDA-BON reach were generally about twice as high as estimates for the LGR-JDA reach (Figures A.2.1-14a to A.2.1-14c). For TURB1 assumptions, absolute differences among models also averaged twice as high (range <1% to 48%; average 3% higher in CRiSP) for the LGR-JDA reach than for the JDA-BON reach (range <1% to 16%; average 1.5% higher in FLUSH) (Figure A.2.1-15a). Absolute differences of $\geq$10% were associated with estimates in 17 years for the LGR-JDA reach but in only one year for the JDA-BON reach.

When the lower river estimate of one model was paired with the upper river of the other model, there was a very small difference in overall in-river survival estimates during most years (Figure A.2.1-16a). However, for the 1982-84 and 1993-96 periods, the FLUSH upstream estimates coupled with CRiSP downstream estimates more closely resembled the original CRiSP estimates than those of FLUSH. These were years in which the downstream estimates were nearly identical in each model and all of the total in-river estimates were similar.

The combination of the above observations suggests that differences among CRiSP and FLUSH model estimates for the JDA-BON reach explain only a small proportion of the differences among CRiSP and FLUSH total in-river survival estimates.

Comparison With Empirical Reach Survival and PIT-tag Detection Estimates

The PATH Hydro Work Group=s Data Subcommittee identified 19 historical reach survival estimates that were potentially useful for evaluating performance of CRiSP and FLUSH passage models in representing historical passage survival (Table A.2.1-2). Additionally, the subcommittee identified four years of LGR-McNary PIT-tag detection (not survival) estimates that would also be useful for evaluations if there were sufficient time.

Table A.2.1-2: Hydropower reach survival and travel time estimates through longest reaches for which methods described in report can be applied.

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1966	0.63	10.0 *229.4*	120.0	IHR Arrivals - TDA Arrivals (Includes free-flowing river through present JDA pool)	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1967	0.64	9.0 *229.4*	132.0	IHR Arrivals - TDA Arrivals (Includes free-flowing river through present JDA pool)	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1968	0.62	17.0 *229.4*	139.0	IHR Arrivals - TDA Arrivals	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1969	0.47	N/A *280.8*	N/A	LMN Arrivals - TDA Arrivals	From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect.
1970	0.22	N/A *326.8*	N/A	LGS Arrivals - TDA Arrivals	Raymond (1979) Table 11
1971	0.48	N/A *97.4*	N/A	LGS Arrivals - IHR Arrivals	Raymond (1979) Table 11

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1972	0.16	N/A *326.8*	N/A	LGS Arrivals - TDA Arrivals	From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect. Slotted bulkheads at LGS.
1973	0.05	22.0 *326.8*	157.0	LGS Arrivals - TDA Arrivals	Survival: Raymond (1979) Table 11. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1974	0.36	12.0 *326.8*	134.0	LGS Arrivals - TDA Arrivals	Survival: From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1975	0.25	12.0 *326.8*	150.0	LGR Arrivals - TDA Arrivals	Survival: From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is wrong. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1976	0.30	15.0 *348.0*	128.0	LGR Arrivals - JDA Arrivals	Survival : Sims et al. (1977) Table 4. Travel Time: Sims et al. (1977) Table 1. Note: Sims and Ossiander (1981) 17 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 15.
1977	0.03	36.0 *348.0*	166.0	LGR Arrivals - JDA Arrivals	Survival : Sims et al. (1978) Table 2. Travel Time: Sims et al. (1978) Table 1. Note: Sims and Ossiander (1981) 39 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 36.
1978	0.47	11.0 *348.0*	131.0	LGR Arrivals - JDA Arrivals	Survival: Raymond and Sims (1980) Table 6, adjusted for LGS transport as in text. Travel Time: Sims et al. (1983) Table 4. Note: Sims and Ossiander (1981) 13 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 11
1979	0.34	13.0 *348.0*	138.0	LGR Arrivals - JDA Arrivals	Survival: Raymond and Sims (1980) Table 6, adjusted for LGS transport as in text. Travel Time: Raymond and Sims (1980) Table 1 and Sims et al. (1983) Table 4. Note: S & O (1981) 15 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O LGR date + 13
1980	0.36	12.0 *348.0*	127.0	LGR Arrivals - JDA Arrivals	Survival: Sims et al. (1981) Table 2. Travel Time: Sims et al. (1981) Table 4 and Sims et al. (1981) Table 1. Note: Sims and Ossiander (1981) 13 day estimate (Table 3) is extrapolated to TDA. Arrival: Sims et al. (1981) [approx. 115] + 12.
1993	0.74	14.4 *91.0*	121.1	Nisqually John Landing - LGS Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Iwamoto et al. (1994) have been revised.

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1994	0.62	14.7 *143.0*	127.3	Silcott Island - LMN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Muir et al. (1995) have been revised.
1995	0.63	17.0 *274.0*	130.0	Port of Wilma - MCN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Muir et al. (1996) have been revised.
1996	0.58	9.3 *225.0*	?	LGR Tailrace - MCN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report.

Comparison With Empirical Reach Survival Estimates

The 19 reach survival estimates included 2-6 projects with various dam configurations and reservoir conditions, which resulted in a range of survival estimates (Figure A.2.1-17). The original observations can be displayed in various ways (e.g., Figures A.2.1-18a to A.2.1-18d), but interpretation is difficult because dam and reservoir effects are confounded. CRiSP and FLUSH estimates based on the various TURB assumptions partitioned the reservoir and dam effects differently, and results of each assumption are compared. Additionally, model estimates varied by whether the models were used in the Aprospective mode@ (i.e., estimates were based only on the types of information that are available for prospective analysis, such as monthly or bi-monthly flows and seasonal project operations) or the Aretrospective mode@ (i.e., estimates were based on additional information, such as daily flows, temperatures, fish distributions, and project operations that are available from the historical record).

AProspective Mode@ Estimates

The fit, expressed as r^2, of each model under each TURB assumption to the empirical estimates is displayed in Table A.2.1-3. [Note: 1966-69 not provided for FLUSH, so n=15 for FLUSH and n=19 for CRiSP]. The FLUSH travel time estimates fit more closely than the CRiSP travel time estimates, the TURB1 and TURB5 survival estimates had very similar fits, and the CRiSP TURB4 estimates fit more closely than the FLUSH TURB4 estimates. FLUSH fits were nearly identical under each TURB assumption because the FLUSH model was re-calibrated for each assumption. Differences among TURB assumptions was greater for CRiSP, which was calibrated with independent data. The TURB4 assumption provided the closest fit to historical data with the CRiSP model.

When compared with the empirical estimates on an annual basis, the years with greatest discrepancies were dependent upon model and TURB assumption. For years prior to 1980 (Figures A.2.1-19a to 19c), FLUSH discrepancies >10% occurred in 1978-80 for TURB1 and TURB5 and in 1970, 1978, and 1979 for TURB4. [Note: FLUSH fit to 1966-1969 is unknown. This is a period of great interest, since it preceded most of the Snake River dams.] CRiSP discrepancies >10% for this time period occurred in 1972, 1973, 1976, and 1977 for TURB1 and TURB5 and in 1976 and 1978 for TURB4. For years since 1993 (Figures A.2.1-20a to 20c), when empirical estimates are based on PIT-tag mark-recapture studies, FLUSH discrepancies >10% occurred in 1990 and 1995 for TURB1 and TURB5 and in 1995 only for TURB4. CRiSP discrepancies >10% occurred in 1994 for all TURB assumptions.

Table A.2.1-3: Summary of linear regression fits (r^2) to empirical reach survival estimates for CRiSP and FLUSH model estimates. FLUSH retrospective mode implemented observed fish travel times (FTT), so comparisons are not applicable. CRiSP comparisons are for 1966-1980 and 1993-1996 (n=19); FLUSH comparisons are for 1970-1980 and 1993-1996 (n=15).

	FLUSH	CRiSP
Prospective Model		
FTT	0.71	0.69
TURB1	0.73	0.72
TURB4	0.73	0.85
TURB 5	0.74	0.77
Retrospective Model		
FTT	N/A	N/A
TURB1	0.85	0.83
TURB4	0.77	0.87
TURB5	0.86	0.84

We intend to make further comparisons of CRiSP and FLUSH, breaking down reach survival estimates into dam and reservoir survival. Figures to be added:

- Figures 21a-c (CRiSP and FLUSH dam survivals by year for each TURB);
- Figures 22a-c (CRiSP and FLUSH reservoir survivals by year for each TURB); and
- Figure 23a-c (CRiSP and FLUSH reservoir survival vs. FTT for each TURB)

ARetrospective Mode@ Estimates

The fits to empirical estimates are better, due to the more detailed in-season information used from the historical record as input to the models.

Comparison With 1989-1992 PIT-tag Detections

[To be written.]

Table A.2.1-4: Comparison of FLUSH and CRiSP model predictions with Lower Granite Dam to McNary Dam PIT-tag detection rates in 1989-1992.

Observed	FLUSH	CRiSP
1989	0.077	0.076
1990	0.080	0.054
1991	0.054	0.033
1992	0.117	0.034

Proportion Transported and Total Direct Survival to Below Bonneville

This section examines CRiSP and FLUSH model estimates of the proportion of Lower Granite pool arrivals that are transported, which is dependent upon model estimates of both dam and reservoir survival and dam passage routing. This information is then combined with an assumed barge survival constant and previously-described in-river survival estimates to develop estimates of total direct survival to below Bonneville Dam.

Proportion Transported

Each model estimated the proportion of fish arriving at the head of Lower Granite pool which were subsequently transported from all collector projects operating during each year. This estimate differs from the "proportion transported" (P) passed to the prospective models, which represents the proportion of survivors below Bonneville that were transported.

Both models agreed that no fish were transported in 1970 and 1974 and both models showed similar long-term temporal patterns (Figures A.2.1-24a to 24c). In general, the proportion transported was 20% or less through 1975-1976, the proportion increased rapidly from 1975-76 to 1980-81, and has generally fluctuated between about 50-70% since that time. However, the short-term temporal patterns and estimates of the actual proportions transported differed by TURB assumption and passage model (Figures A.2.1-24a to 24c and Figure A.2.1-25). For TURB1 and TURB5 assumptions, FLUSH estimated <1% to 25% higher proportions transported than did CRiSP in all years except 1971, 1972, and 1996. Years with passage model discrepancies >10% for both TURB1 and TURB5 included 1979-1983, 1985-1990, and 1994. In general, TURB4 estimates were lower than TURB1 or TURB5 estimates and the differences among passage models were reduced. For TURB 4, CRiSP estimated higher proportions transported than did FLUSH in 1971-73, 1976-79, 1992, and 1996. FLUSH estimated higher proportions in all other years except 1970 and 1974. Discrepancies >10% occurred only in 1980-83 and 1994.

Direct Survival to Below Bonneville

The PATH Hydro Work Group Data Subcommittee reviewed the limited available information and concluded that mortality on barges was likely very low. An assumption of 98% survival of barged fish was recommended for PATH analyses. When the proportion transported is multiplied by this assumed survival rate and the result is added to the product of estimated in-river survival (Figures A.2.1-16a to 16c) and the proportion not transported, an estimate of total direct survival to below Bonneville Dam is obtained:

$$\text{Total Direct Survival} = [\text{Transport\%} * 0.98] + [(1\text{-Transport\%}) * \text{In-River Survival Rate}]$$

Total Direct Survival is different from "System Survival" (o in Delta prospective model; $\exp[-M][DP+1-P]$ in Alpha prospective model), which incorporates assumptions regarding post-Bonneville mortality of transported and in-river migrating smolts. Total Direct Survival corresponds to $\exp[-M]$ in each prospective model.

The pattern of total direct survivals (Figures A.2.1-26a to 26c) is very similar to the pattern of in-river survival (Figures 16a-c) prior to 1975, when there was little or no transportation. After this point, the pattern is nearly identical to that of the transported proportion (Figures A.2.1-24a to 24c). TURB assumptions made little difference (generally <5%) in the disparity between FLUSH and CRiSP direct survival estimates, except during 1971-72 and 1977-79 (Figure A.2.1-27). URB4 assumptions generally resulted in greater differences between FLUSH and CRiSP estimates than did TURB1 or TURB5 assumptions. For most years, FLUSH estimates of total direct survival were higher than CRiSP estimates. Exceptions were in 1973, 1992, and 1996 for all TURB assumptions, as well as 1971 and 1977-79 for

TURB4 assumptions. The most striking discrepancies among models were in 1971 and 1994, when FLUSH estimates were 18-33% higher than CRiSP estimates.

Comparisons of Passage Models' Retrospective Simulations: Main Conclusions

Differences Among Models That Do Not Appear to Be Related to Alternative Hypotheses

We expected that the two passage models would produce nearly identical estimates for all functions except the reservoir survival (vs. FTT, predation, or TDG) functions. However, a number of striking differences among the FLUSH and CRiSP passage model estimates were not related to the alternative reservoir survival hypotheses. These include:

a. Dam passage survival estimates: From 1980-96, CRiSP estimates were consistently higher than FLUSH estimates by an average of about 5% (absolute). This represents about a 7-10% relative difference, since the total dam passage survivals estimated during this period were approximately 50-70%. Particularly troubling are the CRiSP vs. FLUSH differences since 1990, when we presumably have the best information on dam configurations and operations, which are all >5% (absolute) apart. Modeling groups should choose a year of great discrepancy, such as 1994, and carefully examine assumptions on a project-by-project basis to determine if the source of disagreement stems from: 1) alternative hypotheses about dam configuration or operations; 2) alternative hypotheses about fish distribution and timing at projects; or 3) errors that can be corrected.

Differences among models for a given TURB assumption also occurred in the pre-1980 period. Because our knowledge of configuration and operation is not as complete for this period, it would not be surprising if alternative hypotheses for dam configuration or operations exist. However, these have not been articulated, except as the three alternative TURB hypotheses, so discrepancies among models for a given TURB assumption are unexpected. Again, modeling groups should carefully examine assumptions on a project-by-project basis for years such as 1975 or 1972 to try to determine the cause of the discrepancies and whether or not this cause can be expressed in the form of alternative hypotheses.

b. Water Travel Time and Fish Travel Time. Differences among models for these estimates were not expected, but FLUSH WTT estimates (in all years) and FTT estimates (in all but two years) were lower (i.e., faster) than CRiSP estimates. The difference in FTT estimates averaged 4.5 days, which would translate into about a 5-10% difference in reservoir survival at 20-35 day total travel times in the TURB 1,4, and 5 FLUSH models (Figure A.2.1-8). This is a very significant proportion of the total FLUSH reservoir survival estimates at those travel times (approximately 5-35% total reservoir survival).

Modelers should carefully examine the functions used to estimate WTT and FTT to determine if the model discrepancies result from error or fundamental differences in hypotheses. If the latter, those hypotheses need to be articulated and, if the former, the models need to be brought into compliance with each other.

Differences Among Models That Are Related to Alternative Hypotheses

We expected that the two passage models would differ in their reservoir survival functions, due to structural difference in the models (Section A.2.1). The differences are substantial, and vary with TURB assumptions about past dam mortality (Figures A.2.1-8 and A.2.1-9). These results need to be generated for CRiSP with no gas mortality and no predation mortality to better understand differences between the models. Large amounts of spill in some past years likely killed fish in the CRiSP model, causing the variation in survival in Figure A.2.1-8. Because both models have similar fits to the original reach survival observations, one cause of the discrepancy may be the manner in which each model partitioned the reservoir and dam mortality in the empirical reach estimates. This was supposed to have been

controlled by the TURB assumptions but, as seen with the disparity among dam survival estimates for each TURB assumption (above), the models must either be implementing different configurations and operations or the differences in fish distributions are placing the peak of the run at each project when different conditions are in effect. Until each modeling group produces the 19 reach estimates broken down by dam and reservoir survival, we will not be able to evaluate this possibility. If partitioning of survival is the cause of the discrepancy, we will either have an opportunity to bring the models into greater consistency (if the conditions and operations can be agreed upon) or we will have to look beyond the TURB hypotheses and reservoir survival fitting hypotheses to explain the differences.

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Comments on Section A.2.1 - Passage Models by Williams and Responses by Schaller, Petrosky, Wilson and Weber:

Evidence for the flow-survival relationship used in the models and justification for their use in prospective modeling:

i.   FLUSH was based on agreed upon survival estimates from the past, and the curve was the best relationship to fit the yearly data points of survival with concurrent flows. The curve suggests that survival exponentially decreases the longer the fish are in the river. The shape of the exponential decrease is driven by survival estimates in 1973 and 1977.

ii. CRiSP discounts the 1973 and 1977 data as good predictors of reservoir survival because survival in those years was so influenced by problems at the dams. The CRiSP model presumes a constant rate of survival at all flows, thus as travel time increases, overall survival decreases but at a linear rate. The level of daily survival is based on fits of the curve to past data.

iii. Both models presume that they are the best predictor of future reservoir survivals given flows equal to those that occurred in the past.

Response by Schaller et al.: Statements i and ii mischaracterize the 1973 and 1977 data points. PATH has accommodated the concerns about dam survival versus reservoir survival in those two years with TURB1, TURB4 and TURB5 sensitivities. FLUSH fits to TURB1, 4 and 5 all show a negative relationship between reservoir survival and fish travel time, with and without the 1973 and 1977 data points.

(Comments by Williams continues):

Evidence against use of the flow-survival relationships used in the models:

FLUSH Model -

i.   General agreement exists that there is a relationship between flow and travel time for spring/summer chinook salmon. However, when Figure 18c is redrafted in terms of mean survival per km vs. mean travel time (km/day), and the 1973 and 1977 points are excluded, there is no clear relationship. Survivals were substantially lower in 1973 and 1977 even at speeds that are higher than in some other years. This suggests that conditions in those 2 years do not represent anything that will occur in the future.

ii. The predicted (and agreed upon) in-river survival in 1973 was only 4%. The low value was assumed the consequence of low flows and poor passage conditions at Snake River dams. However, approximately 45,000 adults returned from the 1973 outmigration. If in-river fish had only a 4% survival, then the smolt to adult return (SAR) for fish that made it to below Bonneville Dam was nearly 20%. This is highly unlikely. Adjusting the number of alive smolts to below Bonneville Dam to get a SAR of 6% (high end of historical averages) would then, based upon the number of smolts that started the migration at Little Goose Dam, through the river system, provide a survival estimate of approximately 15% or nearly 4 times higher than predicted. This suggests that the 1973 data point is not a correct one on which to base a flow-survival relationship. It leaves only 1977 as one outlier on which to base the curve.

Response by Schaller et al.: The argument that in-river survival in 1973 was not 4%, but rather 15% is flawed in several respects and is undocumented. Williams states that 45,000 adults returned from the 1973 outmigration, but gives no source. Given all the sources of

variation in an aggregate spring/summer chinook recruit estimate, it doesn't seem that a single year's SAR estimate has any bearing on the survival estimate from mark experiments. Williams states that if in-river fish had only a 4% survival, the SAR for fish that made it below Bonneville was 20% which would be unlikely compared to a high of 6% from pre-1970. However, Raymond (1988) indicates wild adult returns of 11,000 (7K springs and 4K summers) from the 1973 migration, which implies about 5% SAR for smolts below Bonneville, more in line with the pre-1970 estimates.

iii.   Survival of PIT-tagged fish released at the Snake River trap to McNary Dam in 1992 was a minimum of 40% (using techniques that subtract out not only transported fish, but fish "destined for transport", the survival was likely 45-50%.) The FLUSH model predicted survivals of fish to John Day Dam in 1992 of 8-16%. Even presuming that survival in the John Day reservoir was only 75%, the FLUSH survival estimate to McNary Dam averaged only 33% of the true survival. Flows in 1992 were nearly the same as in 1973, the difference was that the peak in 1973 was higher but the timing of the peak was later. Travel time through the Snake River was the same for fish in both 1973 and 1992. This suggests that the model is not a good predictor of survivals that will occur under low-flow conditions.

Response by Schaller et al.: The comparison of 1973 and 1992 migration years is not as straight forward as implied. The statement that 1992 and 1973 flow years were similar is incorrect, particularly for the peak yearling migration period. In 1992, 90% of the smolts were trapped before May 6 at the Lewiston Trap. This is the population that was PIT tagged. In 1992 these fish would have experienced flows of 54 kcfs (4/15-5/6 average), compared to only 38 kcfs in 1973. During this period in 1992 water temperatures were in the low 50^oF range. In contrast, in 1973 survival was measured throughout the season and the temperatures in the Snake and Columbia rivers reached 70^oF in the later part of that season.

iv.   Analysis of survivals of PIT-tagged juvenile fish during the 1994-1996 outmigrations found no relationship of within-year flow to survival. No relationship was found even in years when flows varied from as low as 45 kcfs to as high as 180 kcfs for different release groups.

v.   Based on the preliminary results from fish released at Lower Granite Dam in 1995, there was no positive relationship between flow experienced by the juveniles and subsequent adult returns. If anything, the lowest SARs occurred during the periods with the highest flows.

Response by Schaller et al to (iv) and (v).: Williams neglects to identify a key assumption of the within-year analyses of flow and survival: that the different groups were independent. The groups which were assumed independent likely experienced much the same flow regimes but in different river reaches within years. Also, because flow is hypothesized to affect survival indirectly through other factors (water velocity, stage of smoltification, temperature, predation rate,--e.g., ISAB 1996--Return to the River), it is not surprising that a flow-survival relationship was not evident within-seasons. However, there appears to be a flow-survival relationship amongst years (independent groups) for PIT tagged spring chinook.

CRiSP Model

i.   The CRiSP model apparently presumes there is a constant survival per distance traveled, but the value changes with predator densities or consumption rates. The longer the fish are in the system, the lower their survival so any bias relates to the values assumed for constant. If the value for the survival constant in CRiSP is set independent of flow, then it would not be contrary to the empirical results identified (in iv. and v. above under the FLUSH Model).

> *Response by Schaller et al.:* Williams states that the CRiSP model apparently assumes there is a constant survival per distance traveled. We think he meant that CRiSP assumes a constant mortality per day. It is not apparent from the diagnostics that it is constant, because if Figure 8 were expressed for predation only (excluding gas mortality), CRiSP would seem to show a similar relationship as FLUSH TURB4. In fact, predation mortality in CRiSP is temperature dependent and therefore would not be constant over time.

End of comments on Section A.2.1 – Passage Models by Williams and Responses by Schaller et al.

A.2.2 Fish Guidance Efficiency

This section provides a detailed description of the assumptions used for historical and future fish guidance efficiencies.

Guidance Structures

Over time, standard-length guidance screens have been replaced by lowered STS or ESBS at many lower Snake and lower Columbia River mainstem dams. Lower screens deflects more of the intake flow upward, improving fish guidance into the gatewell and the bypass system (Engineering Hydraulics, Inc. 1983 and 1984 and Davidson 1989, cited in McComas et al. 1994). Turbine operating gates have been removed or raised (from the stored position), also to enhance the flow of water up into the gatewell. Although other modifications have been evaluated, these two (lowering or extending the guidance screens further into the powerhouse intake flow and removing or raising the operating gates) have had the largest, most consistent positive effect on FGE.

The configurations of guidance structures at each project over time were determined by reference to a number of sources:

- Time line in the Juvenile Fish Transportation Appendix (C-2, p. 1-16 to 1-24) accompanying Bonneville Power Administration's (BPA) Columbia River System Operation Review (BPA 1994)

- Memo listing installations of fish guidance screens at Corps of Engineers Dams (John McKern, U.S. Army Corps of Engineers Walla Walla District, memo dated May 9, 1997)

- Detailed Fish Operating Plans for the years 1984, 1986 through 1989, 1991, and 1994 (CBFWA 1984, 1986, 1987, 1988, 1989, 1991, and 1994)

- Fish Passage Plans for 1992, 1993, and 1995 through 1997 (Corps 1992, 1993, 1995, 1996, and 1997)

- Reports of the Fish Transportation Oversight Team for FY 1981 through FY 1992 (NMFS 1982, 1983, 1984, 1985, 1986, 1987, 1988, 1989, 1990, 1991, and 1992)

- Annual Reports for the Juvenile Fish Transportation Program for FY 1993 and 1994 (Corps 1995 and 1996)

- Pers. comm. from the staff of NMFS' Hydropower Program, Portland, Oregon, the Portland and Walla Walla District offices of the Corps of Engineers, and the Fish Passage Center

PIT-Tag versus Fyke Net Estimates of Guidance Efficiencies

Data on fish guidance efficiencies have been obtained with fyke-net tests in the past and with PIT-tag studies in more recent years. Fyke-net estimates of FGE are thought to be biased upward because these experiments are conducted in the early evening or nighttime hours when juvenile fish are likely to be distributed in the upper water column (see review in *Return to the River*, Chapter 6, Independent Scientific Group 1996). Fish nearer the surface are likely to be guided by submerged screens, leading to higher estimates of FGE (Askren 1995). During other parts of the day, fish passing dams are distributed deeper in the water column and are less likely to be guided, producing lower FGEs. In addition, fyke-net studies usually evaluate the FGE in only one or two turbine units of a given powerhouse. In contrast, data from PIT-tag detections reflect fish guidance levels on a 24 hour/day, project-wide basis and are likely to provide a more representative estimate of FGE. For these reasons, we give precedence to estimates of FGE derived from PIT-tag studies, where available.

Sensitivity Analysis for the Effect of Extended-Length Screen on FGE

Two alternative hypotheses were derived for the effect of extended-length submersible screens on FGE, to be treated as sensitivities in the model runs:

Sensitivity #1: Assume that extended-length screens have significantly improved FGE. We derive support for this hypothesis from side-by-side (fyke-net, downstream slot) tests of guidance efficiencies for yearling chinook with STS and ESBS at McNary, Little Goose, and The Dalles Dams (**Table A.2.2-1**). In each case, the guidance efficiency of extended-length screens, which fish the upper 2/3 of the turbine intake, was significantly higher than that of standard screens, which fish only the upper 1/3 of the intake.

Table A.2.2-1: Comparison of yearling chinook guidance by standard traveling screens and submersible bar screens in side-by-side comparisons.

Dam	STUDY Year	% FGE Sts[1]	% FGE Esbs[2]	P-Value	PROPORTION POTENTIAL Improvement[3]	Reference
McNary	1992	61	80	--	(1.00 - 0.80)/(1.00 - 0.61) = 0.49	McComas et al. 1993
The Dalles	1993	44	73	$P < 0.001$	(1.00 - 0.73)/(1.00 - 0.44) = 0.52	Brege et al. 1994
Little Goose	1993	74	90	$P < 0.05$	(1.00-0.90)/(1.00 - 0.74) = 0.62	Gessel et al. 1994

[1]Standard-length traveling screens

[2]Extended-length submersible bar screens

[3]Proportion of fish not guided by standard-length screens that would be guided by extended-length screens (assume that FGE_{max} = 100%).

Project-specific observations of FGE for ESBS were not available for Lower Granite Dam. Therefore, we used the data in **Table A.2.2-1** to derive a function relating the FGE of ESBS to that of STS. Based on these tests, extended-length screens guide between 49% and 62% of the fish that are not guided by standard screens. Using the low (conservative) end of that range (i.e., 50%), we suggest the following relationship between FGE with standard vs. extended-length screens for Sensitivity #1:

$$\text{FGE}_{\text{ESBS}} = \text{FGE}_{\text{STS}} + 0.5(1 - \text{FGE}_{\text{STS}})$$　　　　　　　[Eq. A.2.2-1]

where:

　　　FGE_{ESBS} = FGE for yearling chinook with ESBS
　　　FGE_{STS} = FGE for yearling chinook with STS

In effect, extended-length screens guide half the fish that are not guided by the standard-length screens. This adjustment is applied to PIT-tag estimates of FGE for STS (for wild fish) measured at Lower Granite and Little Goose Dams to estimate guidance efficiency under the current configuration (i.e., ESBS). That is, estimates of guidance efficiency for STS, obtained by the "0% spill" method (Smith 1997), are corrected for the effect of extended-length screens.

Sensitivity #2: Assume that extended-length submersible screens have had no effect on fish guidance efficiency. Support for this hypothesis is derived from Russ Keifer's (Idaho Department of Fish and Game) analysis of PIT-tag detections for wild yearling chinook released from Snake River traps during 1993 through 1996 (see Smith 1997). Kiefer's analysis indicates considerable overlap between detection rates at Lower Granite Dam during 1996 (when extended-length screens were in place) and during 1993 through 1995 (standard-length submersible traveling screens in place), at a given spill level. However, because 1996 was a high flow year, Kiefer's method of estimating FGE requires extrapolation of a presumed linear regression function (for probability of detection against % spill) to 0% spill. The 0% spill level is well below the range of observations in data sets for either 1996 or 1997.

Other Correction Factors

Correction for Fyke-Net Position

Until 1992, FGE studies for STS positioned the fyke-net array (used to capture unguided fish) directly under the screen in the bulkhead (upstream) slot. When extended-length screens were introduced, their size precluded direct attachment of the net array. The fyke nets used in ESBS tests were deployed downstream, in the turbine operating gate slot. Subsequent studies conducted by NMFS showed that fish guidance may have been biased upward by the former, upstream position of the nets. Positioned directly under the screen in the upstream slot, the fyke nets may have created a pressure field near the tip of the screen which enhanced guidance into the gatewell (Williams et al. 1996). Positioned in the downstream slot, the nets were less likely to affect the flow field as far forward as the tip of the guidance screen. A correction factor for net position, based on tests conducted at McNary Dam during 1979 and 1992 (Krcma et al. 1980, McComas et al. 1993) was developed. Both of these tests employed a 20-foot long STS attached to a gatewell slot (operating gates fully raised or removed). This correction factor (i.e., the ratio of FGE derived with the fyke-net array in the downstream slot to that derived with the array in the upstream slot) was used to reduce estimates of FGE based on tests conducted with the array in the upstream slot:

$$\text{COR}_{\text{pos}} = \frac{\text{FGE}_{\text{dnstrm}}}{\text{FGE}_{\text{upstrm}}} = \frac{0.61}{0.745} = 0.82$$　　　　　　　[Eq. A.2.2-2]

where:

　　　COR_{pos}　　= correction to downstream position of the fyke-net array
　　　$\text{FGE}_{\text{upstrm}}$ = FGE with nets in the upstream slot, measured at McNary (1979)
　　　$\text{FGE}_{\text{dnstrm}}$　= FGE with nets in the downstream slot, measured at McNary (1992)

Correction for Position of the Operating Gate

During tests of a particular guidance configuration at a given dam, the operating gates may have been raised or removed only for the test period; the guidance structures were otherwise operated with the gates stored. Or, the reverse may have been true: the gates were stored during testing and the guidance structures were otherwise operated with the gates raised or removed. Because side-by side fyke-net tests showed that the position of the operating gate can have a significant effect on FGE (**Table A.2.2-2**) a correction for this effect was developed:

- Brege et al. (1988) tested STS with raised operating gates at Ice Harbor. Subtract 15.3% from his reported estimate of FGE to approximate the value that would have been measured if the gates had been stored. This correction was used to estimate FGE for Ice Harbor during 1993 to 1995.
- McComas et al. (1993, 1994, and 1995) tested ESBS at McNary Dam with the operating gates in the stored position. Add 15.3% to his estimates of FGE to approximate values that would have been measured if the gates had been raised. This correction is used to estimate FGE for McNary during 1997. Other tests at McNary were similarly adjusted.

Table A.2.2-2: Effect of raised operating gate on guidance of yearling chinook in side-by-side comparisons.

Test Location	Year	Screen	Net Pos.	% FGE SOG	% FGE ROG	Increase w/ ROG	P-value	Notes	Reference
LGR	1983	STS	upstrm	55.0	74.0	19.0	*?	--	Swan et al. 1984
LGO	1986	STS	"	61.0	73.5	12.5	<0.005	--	Swan et al. 1987
MCN	1982	STS	"	83.0	88.0	5.0	ns?	BFVBS	Krcma et al. 1983
BON I	1977	ESBS	"	18.0	54.0	36.0	*?	65% porosity screen	Krcma et al. 1978
				55.0	77.0	22.0	*?	35% porosity screen	
BON I	1989	ESBS	"	41.0	43.6	2.6	ns	bar screens w/ perf. plate	Gessel et al. 1990
BON I	1991	STS	"	28.9	49.5	20.6	0.01	--	Monk et al. 1992
BON I	1993	STS	"	38.0	43.0	5.0	ns	--	Monk et al. 1993
					Mean Increase w/ ROG =	15.3			

Correction for Guidance of Wild Versus Hatchery Fish

With few exceptions, PIT-tagged wild chinook were more likely to be detected than hatchery fish for all combinations of year, release site, and detection site (Table 1 in Smith 1997). Overall, wild fish were 18% more likely to be detected than hatchery fish at Lower Granite Dam, 20% more likely at Little Goose Dam, and 16% more likely at Lower Monumental Dam. This relationship is so consistent that it is reasonable to adjust the FGE of the mixed (hatchery+wild) run, observed during fyke-net tests, to that of wild fish by the overall mean of these probabilities (i.e., 18%).

For three of the lower Snake River dams[1], the FGE for the mixed run is adjusted by the following equation:

$$FGE_{Mixed} = (\% \ Wild) * [1.18(FGE_{Hatch})] + (\% \ Hatch) * [FGE_{Hatch}] \qquad \text{[Eq. A.2.2-3]}$$

where:

FGE_{Mixed} = fish guidance efficiency of the mixed run, observed in the fyke-net test
FGE_{Hatch} = fish guidance efficiency of hatchery smolts
FGE_{Wild} = fish guidance efficiency of wild smolts
%Wild = proportion of the Snake River run made up by wild smolts (from Table 2 in Raymond 1988)
%Hatch = proportion of the Snake River run made up by hatchery smolts from Table 2 in Raymond 1988)

The equation was solved for FGE_{Hatch} and then FGE_{Wild} was calculated as:

$$FGE_{Wild} = 1.18 * (FGE_{Hatch}) \qquad \text{[Eq. A.2.2-4]}$$

This method of estimating the FGE of wild fish assumes that the percentages of wild versus hatchery fish at the first dam encountered, as reported by Raymond (1988) in his Table 2, do not change at subsequent Snake River projects.

Raymond (1988) did not present estimates of the proportions of wild versus hatchery fish in runs after 1984. Because new hatcheries continued to come on line through 1992, we assumed that the proportion hatchery fish in years after 1984 could be estimated by the proportions observed during 1981 through 1984 (i.e., 82.2%). A sensitivity analysis showed that increasing the proportion of hatchery fish in the run up to 100% would only change our estimate of the FGE of wild fish by 1%.

For projects downstream of the confluence of the Snake River with the Columbia, we averaged the percent hatchery fish for the Snake run with that for the mid-Columbia River using the data in Raymond's (1988) Tables 2 and 3. The FGE of wild smolts was then estimated using the equation shown above, substituting in the correct percentages and the FGE_{Mixed} observed at each project.

Guidance Efficiencies for Yearling Chinook Salmon

Point estimates of guidance efficiency at each of four lower Snake and four lower Columbia River projects were derived using the data, sensitivities, and correction factors described above. A representative estimate was calculated for each year a project has been in service (service dates from BPA 1991) under the two sensitivities for the effect of extended-length screens. These estimates of yearling chinook guidance efficiency are shown in **Tables A.2.2-3 and A.2.2-4**, respectively.

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[1]No fyke-net tests of guidance efficiency at Lower Monumental Dam

Table A.2.2-3: Estimated yearling chinook guidance efficiencies at lower Snake and Columbia River mainstem dams. Sensitivity #1: $FGE_{ESBS} > FGE_{STS}$.

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Lower Granite	1975-1976	1 of 3 turbines w/ STS, stored operating gate (OG)	46	Fyke-net estimate (50% FGE; Swan et al. 1983), corrected by a factor of 0.8 for fyke-net position, and corrected for percent wild fish.
	1977	3 of 3 turbines w/ STS, stored OG	46	" "
	1978-1990	6 of 6 turbines w/ STS, stored OG	46	" "
	1991-1994	6 of 6 turbines w/ STS, raised OG (ROG)	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997)
Lower Granite (cont'd)	1995	1 of 6 turbines w/ ESBS, ROG	55	Assume that FGE for this unit is approximately equal to the mean of the 1993-1995 PIT-tag estimate for this project (wild fish) (Table 2 in Smith 1997).
		5 of 6 turbines w/ STS, ROG	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
	1996-1997	6 of 6 turbines w/ ESBS, ROG	78	As above w/ Sensitivity #1: assume that FGE for ESBS is approximately equal to the mean of the 1993-1995 PIT-tag estimates for STS (wild fish) (Table 2 in Smith 1997), corrected for ESBS.
Little Goose	1970	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1971-1976	1 of 3 turbines w/ STS, stored OG	55	Fyke-net estimate (61%; Swan et al. 1987), corrected by a factor of 0.8 for fyke-net position and corrected for percent wild fish.
	1977	2 of 3 turbines w/ STS, stored OG	55	" "
	1978-1992	6 of 6 turbines w/ STS, stored OG	55	" "
	1993-1994	1 of 6 turbines w/ ESTS, ROG	64	Mean of 1993 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
		1 of 6 turbines w/ ESBS, ROG	64	" "
		4 of 6 turbines w/ STS, ROG	64	" "
	1995	1 of 6 turbines w/ 1 ESTS + 2 ESBS, ROG	64	" "
		5 of 6 turbines w/ STS, ROG	64	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	82	As above w/ Sensitivity #1: assume that FGE for ESBS is approximately equal to the mean of the 1993-1995 PIT-tag estimates for STS (wild fish) (Table 2 in Smith 1997), corrected for ESBS.
Lower Monumental	1969-1991	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1992-1997	6 of 6 turbines w/ STS, stored OG	61	Mean of 1994 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
Ice Harbor	1961-1966	None	0	
	1967-1979	6-inch orifices to ice and trash sluiceway	3	3% volitional entry from gatewells through 6-in orifices, corrected for percent wild fish.
	1980-1984	6-inch orifices + 1,200 cfs overflow from forebay	30	3% volitional entry plus 23% "guidance" (measured in 1982) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies with fyke-net capture).
	1985-1992	6-inch orifices + 2,700 cfs overflow from forebay	42	3% volitional entry plus 34% "guidance" (measured in 1987) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies).
	1993-1995	6 of 6 turbines w/ STS, stored OG	66	Fyke-net estimate (78% FGE; Brege et al. 1988b), corrected by a factor of 0.8 for net position, minus 15% for OG in stored position, and corrected for percent wild fish. During this period,

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment

	1996-1997	6 of 6 turbines w/ STS, ROG	71	sluiceway gates opened to create approx. 2,000 cfs skimming flow. Sluiceway guidance
				  efficiency approx. equal to 20% (J. McKern, Corps, Walla Walla).
				Fyke-net estimate (78%, above), corrected by a factor of 0.8 for net position (note: new juvenile
				  bypass channel open, sluiceway gates closed), and corrected for percent wild fish.

McNary	1953-1978	None	0	
	1979	1 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1: Estimate of FGE for ESBS derived from fyke-net studies w/ nets in dnstrm slot (81%, 82%, and 87% FGE in McComas et al. 1993, 1994, and 1995, respectively), corrected for percent wild fish.

		1 of 14 turbines w/ STS, stored OG	55	Average of fyke-net estimates in Krcma et al. (56% FGE; 1980 and 66% FGE; 1983),
				  corrected by a factor of 0.8 for net position, and in McComas et al. (61%; 1993), and
				  corrected for percent wild fish.

	1980	6 of 14 turbines w/ STS, stored OG	55	" "
	1981	12.5 of 14 turbines w/ STS, stored OG	55	" "
	1982-1989	14 of 14 turbines w/ STS, stored OG	55	" "
	1990	2 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above.
		12 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1991-1992	14 of 14 turbines w/ STS, stored OG	55	" "
	1993	1 of 14 turbines w/ ESTS, stored OG	79	Sensitivity #1 for ESBS, as above.
		1 of 14 turbines w/ ESBS, stored OG	79	" "
		12 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1994	1 of 13 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above (14th unit out-of-service).
		12 of 13 turbines w/ STS, stored OG	55	Estimate for STS as above (14th unit out-of-service).
	1995	13 of 13 turbines w/ STS, stored OG	55	" "
	1996	6 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above.
		8 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1997	14 of 14 turbines w/ ESBS, ROG	96	Sensitivity #1 for ESBS, as above, + adjustment for ROG.
John Day	1968-1984	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1985	9 of 16 turbines w/ STS (no operating gates at John Day Dam)	67	Fyke-net estimate (72% FGE; Fredricks and Graves 1997), corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1986	12 of 16 turbines w/ STS	67	" "
	1987-1997	16 of 16 turbines w/ STS	67	" "
The Dalles	1957-1974	Ice and trash sluiceway	2	Sluiceway passage efficiency w/ 6-in orifices drilled into the bulkhead between the gatewells and the sluiceway.

	1975-1997	Ice and trash sluiceway	46	As above, with addition of overflow from the forebay (40% FGE; from mark/recapture tests
				  performed during 1982 and 1983, CBFWA 1984), corrected for percent wild fish. Ice and trash
				  sluiceway began operating for fish bypass during 1975 (G. Johnson, Corps, Portland District)

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Bonneville I	1938-1970	None	0	
	1971-1983	Ice and trash sluiceway	4	Sluiceway passage efficiency w/ 12-in orifices drilled into the bulkhead between the gatewells and the sluiceway (G. Fredricks, NMFS, Portland), corrected for percent wild fish.

	1984-1987	10 of 10 units w/ STS, stored OG	72	Fyke-net estimate (71% FGE; Krcma et al. 1980 and 76% FGE; Krcma et al. 1982), corrected
				  by a factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent
				  wild fish.

	1988-1997	10 of 10 units w/ STS, stored OG	41	Navigation lock construction caused change in hydraulic conditions in the forebay which
				  coincided with a reduction in FGE. Average of fyke-net measurements (42% FGE; Gessel
				  et al. 1990, 38% FGE; Monk et al. 1992, and 39% FGE; Monk et al. 1993), corrected by a
				  factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent wild fish.
				FGE for sluiceway system reduced from 40% to 4% due to reduced flow to the sluiceway after the installation of STS.

Bonneville II	1982-1988	8 of 8 units w/ STS, stored OG	21	Average of fyke-net estimates (19% FGE; Krcma et al. 1984 and 26% FGE; Gessel et al. 1985), each corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1989-1992	8 of 8 units w/ STS, STR, stored OG	54	Fyke-net estimate by Gessel et al. (59% FGE; 1985), corrected by a factor of 0.8 for net position, and corrected for percent wild fish
	1993-1997	8 of 8 units w/ LSTS, STR, alt TIE, stored OG	43	Average of fyke-net estimates (66% FGE; Gessel et al. 1988, 31% FGE; Gessel et al. 1989, 46% FGE; Monk et al. 1994, and 44% FGE; Monk et al. 1995), corrected by a factor of 0.8 for net position, and corrected for percent wild fish.

Table A.2.2-4: Estimated yearling chinook guidance efficiencies at lower Snake and Columbia River mainstem dams. Sensitivity #2 $FGE_{ESBS} = FGE_{STS}$.

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Lower Granite	1975-1976	1 of 3 turbines w/ STS, stored operating gate (OG)	46	Fyke-net estimate (50% FGE; Swan et al. 1983), corrected by a factor of 0.8 for fyke-net position, and corrected for percent wild fish.
	1977	3 of 3 turbines w/ STS, stored OG	46	" "
	1978-1990	6 of 6 turbines w/ STS, stored OG	46	" "
	1991-1994	6 of 6 turbines w/ STS, raised OG (ROG)	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997
	1995	1 of 6 turbines w/ ESBS, ROG	55	" "
		5 of 6 turbines w/ STS, ROG	55	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	55	As above w/ Sensitivity #2: assume that FGE for ESBS is approximately equal to FGE for STS.
Little **Goose**	1970	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1971-1976	1 of 3 turbines w/ STS, stored OG	55	Fyke-net estimate (61%; Swan et al. 1987), corrected by a factor of 0.8 for fyke-net position and corrected for percent wild fish2.
	1977	2 of 3 turbines w/ STS, stored OG	55	" "
	1978-1992	6 of 6 turbines w/ STS, stored OG	55	" "
	1993-1994	1 of 6 turbines w/ ESTS, ROG	64	Mean of 1993 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Little Goose (continued)		1 of 6 turbines w/ ESBS, ROG	64	" "
		4 of 6 turbines w/ STS, ROG	64	" "
	1995	1 of 6 turbines w/ 1 ESTS + 2 ESBS, ROG	64	" "
		5 of 6 turbines w/ STS, ROG	64	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	64	As above w/ Sensitivity #2: assume that FGE for ESBS is approximately equal to FGE for STS.
Lower Monumental	1969-1991	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1992-1997	6 of 6 turbines w/ STS, stored OG	61	Mean of 1994 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
Ice Harbor	1961-1966	None	0	
	1967-1979	6-inch orifices to ice and trash sluiceway	3	3% volitional entry from gatewells through 6-in orifices, corrected for percent wild fish.
	1980-1984	6-inch orifices + 1,200 cfs overflow from forebay	30	3% volitional entry plus 23% "guidance" (measured in 1982) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies with fyke-net capture).
	1985-1992	6-inch orifices + 2,700 cfs overflow from forebay	42	3% volitional entry plus 34% "guidance" (measured in 1987) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies).
	1993-1995	6 of 6 turbines w/ STS, stored OG	66	Fyke-net estimate (78% FGE; Brege et al. 1988b), corrected by a factor of 0.8 for net position, minus 15% for OG in stored position, and corrected for percent wild fish. During this period, sluiceway gates opened to create approx. 2,000 cfs skimming flow. Sluiceway guidance efficiency approx. equal to 20% (J. McKern, Corps, Walla Walla District).
	1996-1997	6 of 6 turbines w/ STS, ROG	71	Fyke-net estimate (78%, above), corrected by a factor of 0.8 for net position (note: new juvenile bypass channel open, sluiceway gates closed), and corrected for percent wild fish
McNary	1953-1978	None	0	

	1979	1 of 14 turbines w/ ESBS, stored OG	55	Average of fyke-net estimates in Krcma et al. (56% FGE; 1980 and 66% FGE; 1983)
				  corrected by a factor of 0.8 for net position, and in McComas et al. (61%, 1993), both
				  corrected for percent wild fish, w/ Sensitivity #2: assume that FGE for ESBS is
				 approximately equal to FGE for STS.

		1 of 14 turbines w/ STS, stored OG	55	" "
	1980	6 of 14 turbines w/ STS, stored OG	55	" "
	1981	12.5 of 14 turbines w/ STS, stored OG	55	" "
	1982-1989	14 of 14 turbines w/ STS, stored OG	55	" "
	1990	2 of 14 turbines w/ ESBS, stored OG	55	" "
		12 of 14 turbines w/ STS, stored OG	55	" "
	1991-1992	14 of 14 turbines w/ STS, stored OG	55	" "
	1993	1 of 14 turbines w/ ESTS, stored OG	55	" "
		1 of 14 turbines w/ ESBS, stored OG	55	" "
		12 of 14 turbines w/ STS, stored OG	55	" "

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
McNary) (continued)	1994	1 of 13 turbines w/ ESBS, stored OG	55	" "
		12 of 13 turbines w/ STS, stored OG	55	" "
	1995	13 of 13 turbines w/ STS, stored OG	55	" "
	1996	6 of 14 turbines w/ ESBS, stored OG	55	" "
		8 of 14 turbines w/ STS, stored OG	55	" "
	1997	14 of 14 turbines w/ ESBS, ROG	72	As above, + adjustment for ROG.
John Day	1968-1984	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1985	9 of 16 turbines w/ STS (no operating gates at John Day Dam)	67	Fyke-net estimate (72% FGE; Fredricks and Graves 1997), corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1986	12 of 16 turbines w/ STS	67	" "
	1987-1997	16 of 16 turbines w/ STS	67	" "
The Dalles	1957-1974	Ice and trash sluiceway	2	Sluiceway passage efficiency w/ 6-in orifices drilled into the bulkhead between the gatewells and the sluiceway.
	1975-1997	Ice and trash sluiceway	46	As above, with addition of overflow from the forebay (40% FGE; from mark/recapture tests performed during 1982 and 1983, CBFWA 1984), corrected for percent wild fish. Ice and trash sluiceway began operating for fish bypass during 1975 (G. Johnson, Corps, Portland District).
Bonneville I	1938-1970	None	0	
	1971-1983	Ice and trash sluiceway	4	Sluiceway passage efficiency w/ 12-in orifices drilled into the bulkhead between the gatewells and the sluiceway (G. Fredricks, NMFS, Portland), corrected for percent wild fish.
	1984-1987	10 of 10 units w/ STS, stored OG	72	Fyke-net estimate (71% FGE; Krcma et al. 1980 and 76% FGE; Krcma et al. 1982), corrected by a factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent wild fish. [FGE for the sluiceway system declined from 40% to 4% due to reduced flow to the sluiceway after installation of STS (G. Fredricks, NMFS, Portland).]
	1988-1997	10 of 10 units w/ STS, stored OG	41	Navigation lock construction caused change in hydraulic conditions in the forebay which coincided with a reduction in FGE. Average of fyke-net measurements (42% FGE; Gessel et al. 1990, 38% FGE; Monk et al. 1992, and 39% FGE; Monk et al. a 1993), corrected by factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent wild fish.
Bonneville II	1982-1988	8 of 8 units w/ STS, stored OG	21	Average of fyke-net estimates (19% FGE; Krcma et al. 1984 and 26% FGE; Gessel et al. 1985), each corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1989-1992	8 of 8 units w/ STS, STR, stored OG	54	Fyke-net estimate (59% FGE; Gessel et al. 1985), corrected by a factor of 0.8 for net position, and corrected for percent wild fish.
	1993-1997	8 of 8 units w/ LSTS, STR, alt TIE, stored OG	43	Average of fyke-net estimates (66% FGE; Gessel et al. 1988, 31%FGE; Gessel et al. 1989, 46%FGE; Monk et al. 1994, and 44% FGE; Monk et al. 1995), corrected by a factor of 0.8 and corrected for percent wild fish.

A.2.3 Turbine and Bypass Survival

Estimates for Prospective Analyses

These estimates represent our best understanding of turbine and bypass survival under current and past conditions. Greatest weight was given to the most recent estimates, particularly those derived from PIT-tag studies.

Current Estimate of Turbine Survival (TURB1): Turbine survival is defined as the proportion of fish surviving direct turbine passage. The method of measuring turbine survival includes any incremental indirect mortality experienced in the tailrace by fish that passed through turbines, above the tailrace mortality experienced by fish passing through other routes. Indirect mortality was defined as that which could result from predation upon fish that become disoriented or stressed by passage through turbines. Indirect turbine mortality is not explicitly considered in reservoir survival functions, but is implicit in the FLUSH reservoir survival vs. FTT relationship.

Nine turbine survival studies published through 1990 at Snake and lower Columbia River dams have been reviewed by Iwamoto and Williams (1993). Turbine survival estimates studies ranged from 80-98%, averaging 90%. The Independent Scientific Group (ISG 1996) reviewed studies published through 1995, including several from mid-Columbia River projects. Recent studies using PIT-tags indicated high variability. The PATH Hydro Work Group (1996) reviewed studies published through 1996 and attempted to resolve some of the discrepancies among PIT-tag survival studies by examining details of methodology. It was determined that the 1993 estimate of turbine survival at Lower Granite Dam (0.82; Iwamoto et al. 1994) was less reliable than the 1995 estimate at the same site (0.93; Muir et al. 1996). Release methodology was greatly improved for the latter estimate. This review also suggested that direct survival estimates determined from balloon tags are similar to PIT-tag survival estimates when the same release methodology is employed. The PATH Hydro Work Group (1996) concluded that turbine survival appears to be ≥ 0.90.

Based on a review of field studies at mid-Columbia projects and Lower Granite Dam, a value of 0.90 (Table A.2.3-1) was adopted as the current estimate of turbine survival (with sensitivity to a range of 0.87, in TURB2, to 0.93, in TURB3). Note that analyses of TURB2 and TURB3 have been postponed indefinitely.

Table A.2.3-1: Current estimates of dam passage survival and routing parameters.

	Current Estimate
Turbine Survival	0.90 [TURB1] (Sensitivity to 0.87 [TURB2] and 0.93 [TURB3])
Spill Survival	0.98
Bypass Survival	(See last row for TURB1 in Table LK6)
Spill Efficiency - Snake projects and McNary	1.0 [SPILL1] (Sensitivity to Equation [2] at LGR, LGS, and LMN [SPILL2])
Spill Efficiency - The Dalles	Equation (1)
Spill Efficiency - John Day, Bonneville	1.0
Direct Transport Survival	0.98
Reduction in Reservoir Mortality Due to Squawfish Removal Since 1990	Two Sensitivities: 0 [PRED1], 0.25 [PRED2]

Current Estimate of Bypass Survival (TURB1): Bypass survival is defined as survival past turbine intake screens, gatewells, orifices, bypass flumes, and, in some cases, dewatering screens, wet separators, sampling facilities (including holding tanks), and bypass outfall conduits. These estimates also apply to juvenile bypass through sluiceways at The Dalles, Ice Harbor, and the Bonneville Powerhouse One during certain years. Bypass mortality is likely to have varied across projects and over time (i.e., as bypass facilities were modified, **Table A.2.3-2**).

Table A.2.3-2: Description of bypass facilities by project and date (from J. McKern, Corps of Engineers, Walla Walla District) with estimates adopted as the current and post-1979 bypass survival parameters as well as those included for one sensitivity to pre-1980 bypass survivals.

1. **Bonneville Dam**
   a) **Powerhouse One**
      (1) 1938 to 1971 – no bypass provided
      (2) 1971 to 1983 – ice/trash sluiceway and orifice bypass (97%)
      (3) 1983 to present – STS, orifice bypass to sluiceway (97%)
   b) **Powerhouse Two**
      (1) 1982 to present – STS, bypass tunnel (97%)

2. **The Dalles Dam**
   a) 1958 to 1971 – no juvenile bypass provided
   b) 1971 to 1977 – 6-inch orifices were drilled from the bulkhead slots to the ice/trash sluiceway (99%)
   c) 1977 to present – overflow into sluiceway plus orifice flow (99%)

3. **John Day Dam**
   a) 1968 to 1984 – imbedded pipe bypass system (97%)
   b) 1984 to present – STS, tunnel bypass system (98%)

4. **McNary Dam**
   a) 1953 to 1968 – no bypass provided
   b) 1968 to 1979 – orifice, ice/trash sluiceway bypass (98%)
   c) 1979 to 1994 – STS, flume, pressurized pipe bypass (98%)
   d) 1994 to 1996 – STS, flume, nonpressurized pipe bypass (99%)
   e) 1997 to present – ESBS, flume, nonpressurized pipe bypass (99%)

5. **Ice Harbor Dam**
   a) 1961 to 1967 – no juvenile bypass provided
   b) 1967 to 1980 – 6-inch orifices drilled from bulkhead slots to ice/trash sluiceway, converting the sluiceway to a juvenile bypass system (99%, based on subsequent studies at The Dalles)
   c) 1981 to 1984 – ice/trash sluiceway operated as bypass with 1,200 cfs combined orifice and overflow spill (99%)
   d) 1984 to 1995 – ice/trash sluiceway operated as bypass with 2,700 cfs combined orifice and overflow spill (99%)
   e) 1996 to present – fully screened juvenile bypass system (99%)

6. **Lower Monumental**
   a) 1969 to 1991 – imbedded pipe bypass system (97%)
   b) 1992 to present – STS and tunnel bypass, nonpressurized flume (99%)

7. **Little Goose Dam**
   a) 1971 to 1979 – imbedded pipe bypass system (97%)
   b) 1979 to 1989 – STS, tunnel, pressurized pipe bypass system (97%)
   c) 1990 to 1996 – STS, tunnel, nonpressurized flume bypass system (99%)
   d) 1997 to present – ESBS, tunnel, nonpressurized flume bypass system (99%)

8. **Lower Granite Dam**
   a) 1975 to 1995 – STS, tunnel, nonpressurized flume bypass system (99%)
   b) 1996 to present – ESBS, tunnel, nonpressurized flume bypass system (99%)

A minimum estimate of mortality can be determined from observations of dead fish in sampling facilities. Concurrent observations of descaling may also be used for inferring injury. **Table A.2.3-3** summarizes the available information on yearling chinook descaling and facility mortality estimates at juvenile sampling facilities since 1981. Information from 1981-1992 at Lower Granite, Little Goose, and McNary Dams is summarized in Ceballos et al. (1993). More recent estimates at those projects and at Lower Monumental and Ice Harbor Dams are reported in Hurson et al. (1995 and 1996), Baxter et al. (1996), and preliminary Corps of Engineers reports (summarized by John McKern). Information from Bonneville Dam through 1995 is summarized in Martinson et al. (1996). Detailed descriptions of sampling methods and descaling criteria and changes in these methods and criteria over time are included in Basham and Garrett (1996). A review of bypass mortality since 1981 is included in Giorgi (1996a).

Table A.2.3-3: Percent descaling and mortality at juvenile fish facilities during years for which data are available. (John Day Dam not included because estimates are based on gatewell samples.)

Project	Chinook	Yearling Chinook	Hatchery Yearling Chinook	Wild Yearling Chinook
Lower Granite				
1981	15.5(0.7)			
1982	8.8(0.8)			
1983	3(0.7)			
1984	3(0.5)	(0.4)		
1985	3.3(0.3)	(0.3)		
1986	3.7(0.3)	(0.3)		
1987	3.1(1.2)			
1988	2.4(0.5)			
1989	2.3(0.9)			
1990	3.6(0.3)			
1991	2.4(0.2)			
1992	4.7(0.6)			
1993			4.5(0.4)	3.9(0.4)
1994			3.7(0.4)	3.6(0.4)
1995			2.7(0.3)	0.9(.02)
1996			3(0.6)	1.5(0.9)
Little Goose				
1981	15.4(1.3)			
1982	26(6.2)			
1983	18.4(2.7)			
1984	7.1(1.5)			
1985	7.9(1.0)			
1986	8.8(0.9)			
1987	8.6(1.8)			
1988	12.7(1.2)			
1989	9.9(1.5)			
1990	6.5(1.1)			
1991	3.4(0.9)			
1992	4.1(0.4)			
1993			4.7(0.3)	3.7(0.3)
1994			6.3(0.5)	4.4(0.8)
1995			4(0.4)	2.5(0.5)
1996			4.3(0.6)	2.5(1.2)

Project	Chinook	Yearling Chinook	Hatchery Yearling Chinook	Wild Yearling Chinook
Lower Monumental				
1993			9.2(0.1)	5.8(0.1)
1994			6.2(0.3)	6.1(0.5)
1995			4.2(0.1)	3.2(0.2)
1996			4.5(0.2)	4.1(0.4)
Ice Harbor				
1996			4.1(0.5)	4.8(0.0)
McNary				
1981		8.7(0.9)		
1982		17.9(1.8)		
1983		11.6(0.5)		
1984		12.6(0.3)		
1985		6.0(0.4)		
1986		7.0(0.5)		
1987		5.5(0.8)		
1988		7.6(1.4)		
1989		9.8(0.4)		
1990		8.2(1.2)		
1991		8.7(0.7)		
1992		8.8(1.9)		
1993		5.6(0.6)		
1994		8.4(1.1)		
1995		11.1(0.1)		
1996		7.8(0.1)		
Bonneville Powerhouse 1				
1988		4.4(0.2)		
1989		4.2(0.1)		
1990		7.0(0.1)		
1991		9.3(0.1)		
1992		4.6(0.2)		
1993		3.9(0.1)		
1994		2.6(0.2)		
1995		6.7(0.1)		
Bonneville Powerhouse 2				
1988		5.2(2.1)		
1989		4.4(3.1)		
1990		5.3(0.7)		
1991		10.0(0.8)		
1992		10.2(1.4)		
1993		7.2(0.7)		
1994		5.1(1.3)		
1995		6.7(0.6)		

Based on a review of field studies at lower Snake and Columbia River dams, estimates of current bypass survival rates ranging from 0.97 to 0.99 per project were adopted **(Table A.2.3-4)**. Additional mortality may be associated with predation at the bypass outfall at some locations, but insufficient data were available to estimate this increment in mortality for yearling chinook salmon. These bypass survival estimates are likely to encompass any delayed mortality due to passage through this route.

Estimates for Retrospective Analyses

Historical Estimates of Turbine and Bypass Survival

Historical estimates of bypass and turbine mortality vary from current estimates for some projects during some years. There was general agreement that, between 1980 and the present, the current estimate of turbine survival (0.90 in TURB1, with sensitivity to a range of 0.87 [TURB2] to 0.93 [TURB3]), and the estimates of bypass survival for TURB1 in **Table A.2.3-4** applied. However, there is less certainty about survival estimates prior to 1980, so several alternative hypotheses were described.

Williams and Matthews (1995) concluded that 1970's estimates of juvenile reach survival were lower than would occur under similar river conditions in the 1990's, due to significantly higher debris loads at the first three Snake River projects and sporadic turbine operations. Debris, especially when coupled with low flows or reduced turbine operations, clogged bypass systems and increased velocities through unclogged areas, resulting in injury, descaling, and high delayed mortality of fish collected for transportation experiments. The Corps began removing debris from the Lower Granite forebay in 1980 and a permanent debris rake was installed the following year. Presumably, dam passage mortality has been reduced since that time.

Additionally, the slotted bulkheads installed in skeleton bays of Little Goose, Lower Monumental, and Ice Harbor Dams in 1972 were operated during April and early May in an attempt to reduce dissolved gas levels. Mortality of juvenile fish passing through these structures was extremely high (at least 50% mortality in a study at Lower Monumental Dam, Long et al. 1972 and Long and Ossiander 1974). Over 50% of the yearling chinook outmigration was present in this section of the Snake River during 1972 when 38-51% of the river flow was passing through the slotted gates (Table 8 in Ebel et al. 1973), suggesting that if fish routing was proportional to river flow, 9.5-12.8% of the run was killed per project by this structure alone during 1972. The Corps removed the slotted bulkheads and it is unlikely that fish have been exposed to this level of bypass mortality during recent years.

Based on a review of juvenile survival under historical debris loads and with slotted bulkheads in skeleton bays at some lower Snake River dams, mortality as a function of descaling has varied over time. Thus, it is reasonable to consider a range of estimates for turbine and bypass survival at some Snake River projects in the years prior to 1980. Four sensitivity analyses were identified for pre-80s survival at the upper two projects (i.e., TURB1, TURB4, TURB5, and TURB6).

TURB1: Bypass survival is a function of the structure(s) in place. Neither turbine nor bypass survival at a given project has changed significantly over time. Turbine survival is 0.90 and bypass survivals are listed in **Table A.2.3-4**.

TURB4: Survival due to passage through these routes is significantly lower than would be predicted based on bypass structure alone. Turbine and bypass survivals are both mathematical functions of mortality due to descaling alone **(Table A.2.3-3)** where the rate of mortality due to descaling is resolved over a period of six days after passage.

Table A.2.3-4A: Bypass survival estimates used in TURB1, TURB4, and TURB5/6.

| | | TURB1=McKern version all years; TURB4=Anderson version pre-'80s; TURB6=Weber version pre-'80s | | | | | | | | | | | | | |
| | | LGR | LGR | LGR | LGO | LGO | LGO | LMO | LMO | LMO | IHA | MCN | J DA | TDA | BON1 | BON2 |
BY	OUTYR	TURB1	TURB4	TURB5/6	TURB1	TURB4	TURB5/6	TURB1	TURB4	TURB5/6	TURB1	TURB1	TURB1	TURB1	TURB1	TURB1
1952	1954															
1953	1955															
1954	1956															
1955	1957															
1956	1958															
1957	1959															
1958	1960															
1959	1961															
1960	1962															
1961	1963															
1962	1964															
1963	1965															
1964	1966															
1965	1967											0.99					
1966	1968											0.99	0.98	0.97			
1967	1969							0.97		0.97	0.99	0.98	0.97			
1968	1970						0.9	0.97	0.97	0.97	0.99	0.98	0.97			
1969	1971				0.97	0.65	0.9	0.97	0.97	0.97	0.99	0.98	0.97			
1970	1972				0.97	0.58	0.834	0.97	0.5	0.97	0.99	0.98	0.97			
1971	1973				0.97	0.51	0.804	0.97	0.51	0.97	0.99	0.98	0.97			
1972	1974				0.97	0.89	0.85	0.97	0.98	0.97	0.99	0.98	0.97			
1973	1975	0.99	0.64	0.87	0.97	0.64	0.8	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1974	1976	0.99	0.79	0.93	0.97	0.68	0.885	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1975	1977	0.99	0.41	0.765	0.97	0.44	0.761	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1976	1978	0.99	0.78	0.93	0.97	0.5	0.8	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1977	1979	0.99	0.84	0.947	0.97	0.76	0.92	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1978	1980	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	

Table A.2.3-4A (cont.): Bypass survival estimates used in TURB1, TURB4, and TURB5/6.

		LGR	LGR	LGR	LGO	LGO	LGO	LMO	LMO	LMO	IHA	MCN	JDA	TDA	BON1	BON2
BY	**OUTYR**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**
1979	1981	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1980	1982	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	0.97
1981	1983	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	0.97
1982	1984	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1983	1985	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1984	1986	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1985	1987	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1986	1988	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1987	1989	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1988	1990	0.99	0.99	0.99	0.99	0.99	0.99	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1989	1991	0.99	0.99	0.99	0.99	0.99	0.99	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1990	1992	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.98	0.99	0.97	0.97
1991	1993	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.98	0.99	0.97	0.97
1992	1994	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1993	1995	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1994	1996	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1995	1997	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97

Table A.2.3-4B: Turbine survival estimates used in TURB1/6, TURB4, and TURB5.

		TURB1=McKern version all years; TURB4=Anderson version pre-'80s; TURB6=Weber version pre-'80s															
	LGR	LGR	LGR	LGR	LGO	LGO	LGO	LMO	LMO	LMO	IHA	MCN	J DA	TDA	BON1	BON2	
BY	OUTYR	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB5	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6
1952	1954																
1953	1955																
1954	1956																
1955	1957																
1956	1958																
1957	1959																
1958	1960																
1959	1961																
1960	1962																
1961	1963																
1962	1964																
1963	1965																
1964	1966																
1965	1967												0.90					
1966	1968												0.90	0.90	0.90			
1967	1969								0.90			0.90	0.90	0.90	0.90			
1968	1970								0.90	0.97	0.45	0.90	0.90	0.90	0.90			
1969	1971				0.90	0.65		0.90	0.97	0.45	0.90	0.90	0.90	0.90				
1970	1972				0.90	0.58	0.85	0.90	0.5	0.855	0.90	0.90	0.90	0.90				
1971	1973				0.90	0.51	0.85	0.90	0.51	0.855	0.90	0.90	0.90	0.90				
1972	1974				0.90	0.89	0.85	0.90	0.98	0.855	0.90	0.90	0.90	0.90				
1973	1975	0.90	0.64		0.90	0.64	0.81	0.90	0.97	0.81	0.90	0.90	0.90	0.90	0.90	0.90	
1974	1976	0.90	0.79		0.90	0.68	0.83	0.90	0.97	0.83	0.90	0.90	0.90	0.90	0.90	0.90	
1975	1977	0.90	0.41	0.84	0.90	0.44	0.81	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1976	1978	0.90	0.78	0.87	0.90	0.5	0.85	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1977	1979	0.90	0.84	0.79	0.90	0.76	0.79	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1978	1980	0.90	0.99	0.87	0.90	0.97	0.81	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	

Table A.2.3-4B: Turbine survival estimates used in TURB1/6, TURB4, and TURB5.

		LGR	LGR	LGR	LGO	LGO	LGO	LMO	LMO	LMO	IHA	IHA	MCN	JDA	TDA	BON1	BON2
BY	OUTYR	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB5	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6
1979	1981	0.90	0.99	0.88	0.90	0.76	0.86	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1980	1982	0.90	0.99	0.88	0.90	0.90	0.86	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1981	1983	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1982	1984	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1983	1985	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1984	1986	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1985	1987	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1986	1988	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1987	1989	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1988	1990	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1989	1991	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1990	1992	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1991	1993	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1992	1994	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1993	1995	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1994	1996	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1995	1997	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
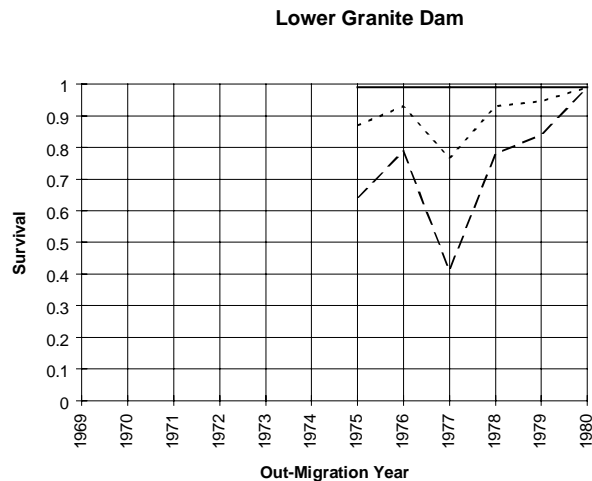
Lower Granite Dam

Figure A.2.3-1: Historical dam passage survival estimates for Lower Granite Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The light dashed line represents the alternative set of estimates designated TURB6. The heavy dashed line represents the set of estimates designated TURB4. See text for descriptions of these alternatives.
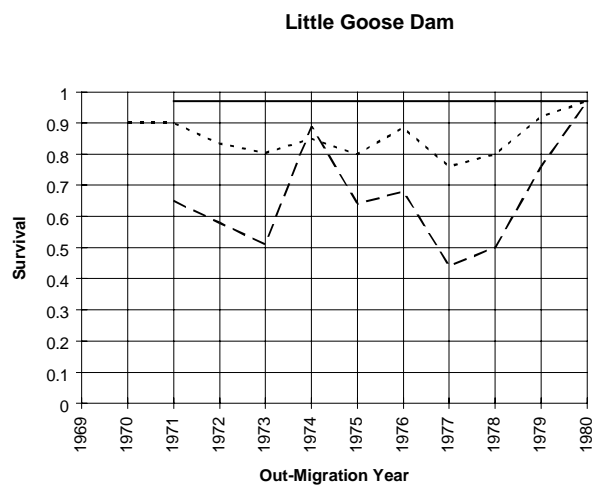
Little Goose Dam

Figure A.2.3-2: Historical dam passage survival estimates for Little Goose Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The light dashed line represents the alternative set of estimates designated TURB6. The heavy dashed line represents the set of estimates designated TURB4. See text for descriptions of these alternatives.

Lower Monumental Dam
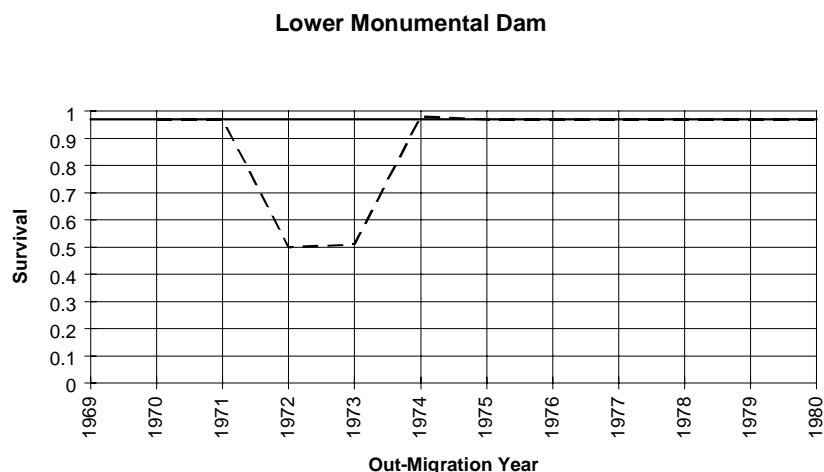
Figure A.2.3-3:   Historical dam passage survival estimates for Lower Monumental Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The dashed line represents the alternative set of estimates designated TURB4. The alternative estimates designated TURB6 were not associated with Lower Monumental Dam. See text for descriptions of these alternatives.

Rationale and Details

Williams and Matthews (1995) stated that descaling due to the presence of forebay trash probably resulted in poor fish survival. Because descaling does not instantaneously kill fish, in order to characterize mortality in studies covering different time periods, the mortality must be expressed by a rate equation of the form:

$$dS/dt = (-r * descaling * S) \qquad\qquad \text{[Eq. A.2.3-1]}$$

Where r is the rate of mortality per unit time and percent of the population with greater than 10% descaling, S is the percent of the population alive, descaling is the frequency of occurrence of descaling, and t is the time of the descaling event and observation. Note that, in this equation, descaling is a measure of the stress experienced by fish. Solving Equation (1), the percent alive at a given time, T, is then:

$$S(t) = S0 * \exp(-r * descaling * T) \qquad\qquad \text{[Eq. A.2.3-2]}$$

where S0 is an intercept term which adjusts the data and in a sense is the survival that would be experienced if descaling was zero. In general, this term is related to the instantaneous mortality, which is expected to be small because mortality from a stress event is generally delayed. The rate coefficient, r, can be obtained by rearranging Equation (2) as a log-linear regression of the form:

$$\log(100 - mortality) = a + r * descaling * T) \qquad\qquad \text{[Eq. A.2.3-3]}$$

where *a*, the intercept term, is the log of the instantaneous survival. Then:

$$\exp(a) = S0 \qquad\qquad \text{[Eq. A.2.3-4]}$$

Using Equations [A.2.3-3] and [A.2.3-4], we can investigate the passage/descaling mortality data sets and estimate the mortality rate parameters.

Observations of mortality from dam passage fall into three categories distinguished by the time between passage and observation (short, intermediate, and long). Studies encompassing short periods (i.e., six hours) are available that identify descaling and mortality of fish observed in the collection facilities at Lower Granite and Little Goose Dams between 1981 and 1993. These measurements were collected as part of the Fish Transportation Oversight Team which monitors fish transportation. In general, the observation time in these studies is expected to be within several hours of the stress event, i.e., entering the forebay of a dam. The short period studies have been reviewed by Giorgi (1996).

Intermediate time period studies (i.e., two days) were reported by Williams and Matthews (1995). These involved measurements of descaling and mortality after holding fish for 48 hours before or after truck transport to an area below Bonneville Dam. These observations were made at Little Goose and Lower Granite Dams between 1972 and 1990 and were also reported in Williams and Matthews (1995).

In the long time period studies (i.e., six days), fish were marked and released above and below a dam and the differential mortality in passage was estimated from collections at downstream dams. Raymond (1979), reported mortalities at Little Goose Dam during 1972 and 1973 and more recently (i.e., 1993), turbine survival measurements were reported from PIT-tag studies for Little Goose and Lower Granite Dams (Iwamoto et al. 1994). Descaling observations were not recorded during these long period studies so descaling estimates for the corresponding years have been obtained from Williams and Matthews (1995) and Giorgi (1996).

Statistics for three regressions of mortality against descaling, using the data sets encompassing short, intermediate, and long time periods, respectively, and for a regression combining the data sets, are shown in **Table A.2.3-5.** The regression equation for the combined data:

$$M(t) = 100 - 99.98 * \exp(-0.0058 * descaling * T) \qquad \text{[Eq. A.2.3-5]}$$

assumes a time period of six days to resolve the effects of descaling. That is, the regression provides a coherent and statistically significant fit of data with time factors that vary from a few hours to six days. This suggests that the model can extrapolate mortality observed over several days. That is, the single rate of mortality coefficient, r, fits mortality data over 0.25 to 6 days.

This finding is supported by studies of plasma glucose levels in juvenile chinook after descaling (Congleton et al. 1997). Initial levels prior to descaling or handling were 50 mg/dl. With descaling or simply handling, the levels rose to > 80 mg/dl. Plasma glucose levels in fish handled but not descaled were still above background after two days. Although the fish were handled after four days, plasma glucose levels still had not returned to background after eight days. In further experiments, these authors followed the cumulative mortality of descaled and intact fish exposed to disease. In both conditions, after a five-day induction period, mortality occurred over five additional days.

Table A.2.3-5: Coefficients and significance levels for regressions describing yearling chinook salmon mortality as a function of descaling assuming that mortality takes place over short, intermediate, or long time periods and for all time periods combined. These relationships are the basis for the survival estimates in TURB4.

Time Period	Days(T)	Sample Size (N)	R^2	P-value	Intercept (std err)	Slope (std err)
Short	0.25	26	0.6656	0	4.6067 (0.0023)	-0.0017 (0.0002)
Intermediate	2	30	0.5744	0	4.6145 (0.0291)	-0.0123 (0.0020)
Long	6	4	0.9459	0.0274	4.6217 (0.0767)	-0.0348 (0.0059)
Combined	--	60	0.8308	0	4.6058 (0.0099)	-0.0058 (0.0003)

[$F = 284.8748$ (1, 58 df), P-value = 0]

Passage mortalities for the lower Snake River dams during the 1970s were estimated from Equation A.2.3-5 and the observed rates of descaling reported in Williams and Matthews (1995) (**Table A.2.3-6**). Descaling, when combined with other stressors, can have a significant effect on mortality over a period of several days. Because descaling levels of 25% have been associated with mortality rates of 50%, it is reasonable to assume that the measure of descaling is a surrogate of stress in dam passage. *Section A.3.1 (Transportation Assumptions in CRiSP) also discusses trends in descaling rates over time.*

Table A.2.3-6: Estimates of turbine and bypass mortality for TURB4 (see text). "% Descaled column can be compared with similar estimates in Table A.2.3-7, which were used for TURB5 and TURB6

Year	Dam	% Descaled	Turbine/Bypass Mortality (%)	Source
1972	LGS	16	42/42	Eq(6)
	LMO	-	50/50	Data for coho salmon (slotted bulkhead) in Raymond (1979)
1973	LGS	19.6	49/49	Eq(6) and Raymond (1979)
	LMO	-	49/49	Raymond (1979
1974	LGS	-	11/11	Using values from Raymond (1979)
	LMO	-	10/2	Assuming same as LGO
1975	LGR	13.0	36/36	Eq(6)
	LGO	-	36/36	Assuming same as LGR
1976	LGR	7.0	21/21	Eq(6)
	LGO	11.5	32/32	Eq(6)
1977	LGR	26.0	59/59	Eq(6)
	LGO	23.9	56/56	Eq(6)
1978	LGR	7.5	22/22	Eq(6)
	LGO	20.0	50/50	Eq(6)
1979	LGR	5.3	16/16	Eq(6)
	LGO	8.1	24/24	Eq(6)
1980	LGR	4.0	13/13	Eq(6)
	LGO	-	10/2	Eq(6)

TURB5: This hypothesis assumes a much lower dam mortality rate than TURB4. During the period of concern, very few fish passed LGR and LGO via the bypass route. Most fish which entered the bypass channel were collected and then transported. Consequently, the high rates of descaling reported for fish encountering screens and blocked or poorly designed orifices have little effect on reach survival estimates of in-river fish.

Because there is some evidence of descaling associated with turbine passage, there is a need to address increased turbine mortality as a sensitivity analysis. Park et al. (1978) indicated that fish sampled in the forebay, which presumably had been swimming in and out of the trash racks, were descaled at a 10-14% rate, approximately half the rate observed for the bypassed fish during 1977. Therefore fish passing through the turbines were assigned a rate of mortality equal to one-half the rate of descaling (**Table 4.2.3-7**) and fish passing through the bypass system (and not transported) have a mortality rate equal to the rate of descaling at a given dam in a given year.

TURB6: Some additional debris-related mortality occurred during early years but survival was higher than would be estimated by TURB4. As bypass conditions improved during the early 1980s, descaling was reduced from double digits to approximately 4%. This rate of descaling continued until recent years. The descaling rates estimated and observed for Lower Granite and Little Goose dams can be applied as the mortality rates for fish that passed these two projects via the bypass route. That is, bypass survival can be estimated as a function of mortality due to descaling, by assuming that the rate of mortality is equal to the rate of descaling. The survival of fish passing through the turbine route would be the same as that described in TURB1 (i.e., 0.90 ± 0.03).

A.2.4   Spill Survival and Spill Efficiency

Spill Survival

Standard Estimate of Spill Survival: The ISG (1996) reviewed estimates of spill survival in the Snake and Columbia Rivers published through 1995. Mortality estimates for 10 of the 13 studies ranged between 0 and 0.022. Estimates from the other three studies were extremely variable (i.e., ranged from 0.04 to 0.275) and should be viewed with caution. In some studies, mortality appears to be higher in spillways with spill deflectors than in those without deflectors, but these differences were generally not statistically significant (e.g., Muir et al. 1995). Additional studies by the Corps are currently underway to resolve this issue.

A value of 0.98 was adopted as the standard estimate of spillway survival. This estimate may be conservative, representing the results of spillway survival studies conducted to with radio-, PIT-, and balloon-tags in spillbays with flow deflectors, operated at discharge rates below 10 kcfs per bay. Survival may decrease as spillbay releases approach flood capacity (about 15+ kcfs), but these conditions have rarely been tested to date. Survival may decrease for juvenile or adult spill patterns adjusted away from the currently slightly-crowned or flat pattern proposed for changing smolt passage distributions for improving spill efficiency. This was the case at The Dalles Dam during 1997, where spill survival was measured at less than 0.8 with a northshore skewed pattern. Uncertainty in spill survival over the range 0.87 to 0.93 was explored in initial passage model runs, but it was apparent from these results that different assumptions about spill survival had very little effect on the model output. Therefore, the analyses presented in this report are based solely on the standard estimate of 0.98.

Table A.2.3-7:   Proportion of descaled fish passing through bypass (i.e., "Prop.") used as the basis of the survival rate estimates in TURB5 and TURB6. These can be compared with similar descaling estimates in Table A.2.3-6, which were used for TURB4.

	Lower Granite			Little Goose		
Year	Units Screened	Prop.	Comments	Units Screened	Prop.	Comments
1970	na	na		0 of 3	0.1	Nominal
1971	na	na		0 of 3	0.1	Nominal
1972	na	na		0 of 3	0.166	
1973	na	na		0 of 3	0.196	
1974	na	na		0 of 3	0.15	Mean of range
1975	0 of 3	0.13		0 of 3	0.2	See Note 1
1976	2 of 3	0.07		0 of 3	0.115	
1977	2 of 3	0.235		0 of 3	0.239	
1978	6 of 6	0.07		6 of 6	0.2	
1979	6 of 6	0.053		6 of 6	0.08	See Note 1
1980	6 of 6	0.04		6 of 6	0.08	
1981	6 of 6	0.154		6 of 6	0.135	
1982	6 of 6	0.082		6 of 6	0.26	
1983	6 of 6	0.028		6 of 6	0.199	

Note 1/ No data. Estimate based on ratio of LGR to LGS.

Spill Efficiency

Standard Estimate of Spill Efficiency: Spill efficiency is defined as a ratio of the proportion of the smolt population passed via the spillway (spill effectiveness) to the proportion (percent) of total flow discharged as spill. Steig (1994) reviewed studies at Snake and Columbia River dams published through 1992 and noted that there is considerable variability in daily and weekly spill effectiveness. However, he concluded that most of the results fall around a 1:1 relationship between the proportion of water spilled and the proportion of fish passed in spill (i.e., 1.0 spill efficiency). Giorgi (1996b) reviewed estimates of spill efficiency published through 1993 and pointed out that efficiencies are poorly estimated for most species due to a combination of sparse observations, imprecise estimates, and the reliance of most estimates on hydroacoustic monitoring, which is unable to distinguish among species. He cautioned that the assumption of a spill efficiency of 1.0 could not be justified in most cases. Giorgi implied that a suite of estimates acquired with different methodologies should be considered when deriving species-specific estimates at individual dams. Relying largely on Giorgi's (1996b) review, we concluded that a range of spill efficiencies from 1-2 should be incorporated into sensitivity analyses at dams in the Snake and lower Columbia rivers.

We agree with Giorgi's (1996b) characterization of the uncertainty associated with spill efficiency estimates, but conclude that, if a single estimate must be chosen, most studies support using a factor of 1.0 at all projects except The Dalles (Table A.2.3-1, Figure A.2.4-1). The Dalles Dam has a significantly different configuration than other projects, with a spillway oriented perpendicular to the natural course of the river and the powerhouse oriented nearly parallel to the course of the river. As stated by the Independent Scientific Group (1996), it is not surprising that this project exhibits higher spill efficiency than many other projects. Therefore, a factor of 2.0 was applied at The Dalles Dam at spill levels $\leq$ 30%. Above 30% spill, the relationship grades from 2.0 to 1.0 according to Equation (1) (Table A.2.3-1, Figure A.2.4-1). This relationship predicts a factor of 1.5 at 65% spill.

(1)

$$P_f = 2.0 * P_w \qquad\qquad 0 < P_w \leq 0.30$$
$$P_f = (2.43 - 1.43 * P_w) * P_w \qquad\qquad P_w > 0.30$$

where:

P_f = proportion of fish passing over the spillway
P_w = proportion of total river flow passing over the spillway

Spill efficiency, is defined as ($P_f \div P_w$). Support for this relationship comes from several sources. Giorgi and Stevenson (1995) reviewed biological investigations that described smolt passage behavior at The Dalles Dam and discussed implications to future surface bypass and collection research. They cited three investigations that indicated that spill efficiency was near 2.0 when about 20% of the flow passed over the spillway. Included among these studies is Willis (1982), which describes a curvilinear relationship in which spill efficiency is $\geq$ 2.0 at spill below approximately 30% of total river flow, the efficiency declines to about 1.4 at 60% spill, and declines to 1.0 at 100% spill. A recent radiotelemetry study at The Dalles by Holmberg et al. (1997) supports this general relationship. Spill efficiency for yearling chinook was 2.3 at 30% spill and 1.25 at 64% spill in 1996.

We also suggest application of a sensitivity analysis to the estimate of spill efficiency at Lower Granite, Little Goose, and Lower Monumental dams if there is sufficient time. The suggested sensitivity analysis for Snake River projects relies on a relationship for spring chinook salmon at Lower Granite Dam that is based on radio-telemetry observations (Wilson et al. 1991). Because of the similarity of the three projects we see no reason to assume that the Lower Granite radio-telemetry results are unique to that project. (Similar studies have not been conducted at other Snake River projects). By combining the radio-

telemetry observations with assumptions that: (1) 0% of fish pass the spillway at 0% spill and (2) 100% pass at 100% spill, the following relationship (Smith et al. 1993, Figure 1) can be applied:

(2) $P_f = 2.583*P_w - 3.250*P_w^2 + 1.667*P_w^3$

where:

P_f = proportion of fish passing over the spillway
P_w = proportion of water passing over the spillway

Spill efficiency, is defined as $(P_f \div P_w)$. As cautioned by the Independent Scientific Group (ISG 1996), the shape of this relationship is highly uncertain outside of the range of observations (20%-40% spill), even though P_f must logically go to 0 and 1.0 at the extremes.
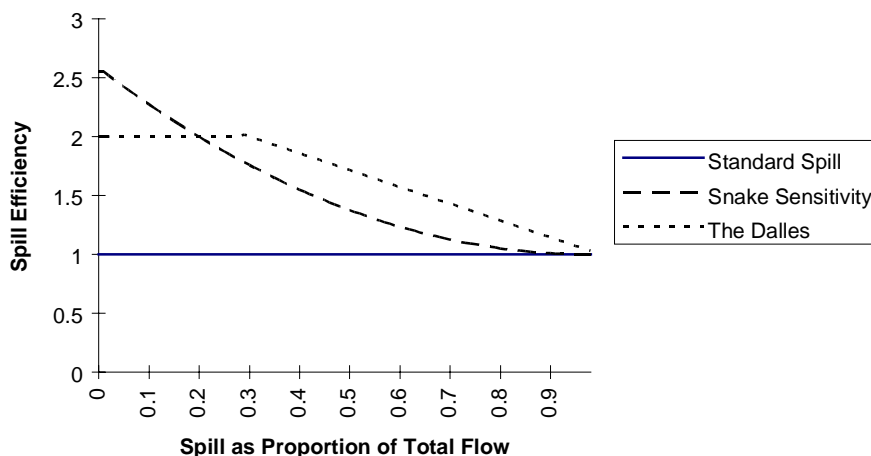
Figure A.2.4-1: Comparison of standard spill efficiency at all projects other than The Dalles, standard spill efficiency at The Dalles, and the sensitivity analysis for spill

A.2.5 Predator Removal Efficiency

Relationships for reservoir survival in FLUSH and CRiSP are based on reach survival estimates that were collected from *?-199?* and on predation research conducted primarily in the 1980s. The survival relationships thus depict the system prior to about *199?*, and system changes that occurred in recent years might not be fully captured in these relationships. Since 1990, the Bonneville Power Administration has funded various programs designed to remove large northern squawfish from Snake and Columbia river reservoirs, and over 900,000 predators have been collected (Beamesderfer et al. 1996). Since northern squawfish are believed to be a major source of mortality for juvenile salmonids (Rieman et al. 1991), changes in predation pressure could have a significant influence on reservoir survival. We estimated the highest and lowest potential effects of squawfish removal on spring/summer chinook survival to use a bounds in sensitivity analyses (25% and 0% reductions in reservoir mortality).

<< next 2 paragraphs need to be edited>>

The higher sensitivity adjustment was estimated first. The average predation -related rate of mortality for salmonids migrating in April-June was estimated to be about 9% of all juvenile salmon entering John Day Reservoir, or 11% as the mean plus 1 SD (Rieman et al. 1991; their Table 6). Early studies suggested that sustained removal *(Reviewer's note: sustained for how long?)* of the northern squawfish from John Day Reservoir would reduce predation-related mortality by 5-% (Rieman and Beamesderfer 1990). Average annual exploitation rates in Columbia and Snake River reservoirs have changed from 9-16% during 1991-1995 (Beamesderfer et al. 1996). Assuming recent removals could be reducing predation by 50%, *(Reviewer's note: Relative to?)* the upper limit for sensitivity adjustment would be 5.5% (0.5 x 0.11 = 0.55). *(Reviewer's note: Methods need to be more clearly stated in this paragraph.)* *<How do these limits relate to the 0 and 25% used in the decision analysis?>*

For the lower sensitivity adjustment, we first reduced by one-third the average April-June mortality caused by predators (adjusted mortality = 7.5.%), *(Reviewer's note: show equation)* which corresponds roughly to predation loss estimates in Petersen (1994) based on finer subdivision of the reservoir. Next, we estimated exploitation *(Reviewer's note: of squawfish?)* as the average for the lower 95% confidence bound of achieved exploitation rates (10%; Beamesderfer et al. 1996; their Table 2). Assuming a 10% exploitation rate would produce a 25% reduction in predation-caused mortality, the lower estimate for survival change in spring/summer chinook would be 1.9% (0.25 x 0.75). Compensatory responses by the northern squawfish that remain the system, or by predaceous smallmouth bass or walleye, could reduce the effectiveness of the predator removal program (Beamesderfer et al. 1996; J.H. Petersen *unpublished analyses*), so we set the lower limit for sensitivity adjustment to 0% (Table 1). *(Reviewer's note: analysis needs clarification.)*

References

Beamesderfer, R.C., D.L. Ward, and A.A. Nigro. 1996. Evaluation of the biological basis for a predator control program on northern squawfish (*Ptychocheilus oregonensis*) in the Columbia and Snake Rivers. Canadian Journal of Fisheries and Aquatic Science 53:2898-2908.

Petersen, J.H. 1994. Importance of spatial pattern in estimating predation on juvenile salmonids in the Columbia River. Transactions of the American Fisheries Society 123:924-930.

Rieman, B.F., and R.C. Beamesderfer. 1990. Dynamics of a northern squawfish population and the potential to reduce predation on juvenile salmonids in a Columbia River reservoir. North American Journal of Fisheries Management 10:228-241.

Rieman, B.E., R.C. Beamesderfer, S. Vigg, and T.P. Poe. 1991. Estimated loss of juvenile salmonids to predation by northern squawfish, walleyes, and smallmouth bass in John Day Reservoir, Columbia River. Transactions of the American Fisheries Society 120:448-458.

A.2.6 Drawdown

Uncertainties related to drawdown were focused on the duration of and survival rates during four time periods:

1. Pre-removal - the period between when the region decides to proceed with drawdown and when physical removal of dams begins.
2. Removal - period in which engineering work to breach or circumvent the dams is carried out.
3. Transition - period beginning just after the dams are removed and continuing until fish populations attain some equilibrated conditions.

4. Equilibrium - period of time from when fish populations equilibrate to the end of the simulation period.

Alternative hypotheses were defined for three uncertainties that the Drawdown workgroup considered to be most important in determining the biological response. These three key uncertainties were:

a) Duration of the pre-removal period
b) Juvenile survival during the transition period
c) Equilibrated juvenile survival rates

a) Duration of the pre-removal period

Uncertainties in the duration of the pre-removal period are related to the Congressional appropriations cycle and the potential for litigation following the regional recommendation. The best-case scenario was considered to be 3 years, which would be the case if there were no litigation and the regional decision was made in time to catch the next appropriations cycle in Congress. The worst-case scenario of 8 years allows for delays due to litigation and potential problems in getting Congressional approval and funding for drawdown.

b) Juvenile survival during the transition period

Batelle's Pacific Northwest Environmental Laboratory is working on modeling timing of physical processes for the Corps, but preliminary results are not expected until Spring 1998. In the meantime, the group made some preliminary estimates of transition timing based on available evidence. Information presented at one of the drawdown meetings included:

Timing of physical responses (Corps estimates):
- sediments in main channel scoured out within 2 years of drawdown
- sediments along banks partially scoured out approx. 10 years after drawdown
- banks stabilized, recolonization by vegetation approx. 10 years after drawdown

Timing of biological responses
- response to removal of dam mortality should be immediate
- redistribution of predators within river could take place relatively quickly
- population responses by predators at least 5 years after drawdown (based on time to maturity for predators), although drawdown is not likely to reduce predator populations

Based on this information, the group decided on 2 years and 10 years as alternative hypotheses about the length of the transition period. The lower bound represents the effects of short-term physical (scouring of main channel) and biological (redistribution of predators) processes. 10 years reflects the effects of longer term physical processes, such as bank stabilization, and predator population responses to drawdown.

Juvenile survival rates could follow any of a number of trajectories during the transition period. Optimistic trajectories would have an immediate increase in juvenile survival following dam removal equal to the amount of dam mortality associated with the 4 Snake River dams. Pessimistic trajectories might involve an initial decrease in survival following dam removal resulting from the release of sediment from the drawdown reservoirs. For the purposes of the preliminary analysis, juvenile survival rates were assumed to simply increase linearly during the transition period, from their initial pre-removal values to their equilibrated values.

c) Equilibrated juvenile survival rates

The group identified two alternative hypotheses about equilibrated juvenile survival rates. These hypothesized rates are based on pre-dam survival estimates and on current estimates of survival in free-flowing reaches of the Snake River above Lower Granite Dam (Table A.2.7-1), expanded to correspond to the 210-km reach encompassed by the four Snake River dams. Although there are numerous caveats involved in using these reach survival estimates to predict equilibrated survival rates (e.g., reach sections in recent estimates have little overlap with reaches in historical estimates and with the reach that would correspond to natural river drawdown), the data in Table A.2.7-1 do provide an empirical basis for defining the bounds of the biological responses we might expect following drawdown.

Based on these data, the group developed 2 hypotheses:

H1:   Survivals will return to 0.96, the level observed as they were prior to the construction of the dams. Implicit in this hypothesis is the assumption that predator densities in the free-flowing river following drawdown will return to historical levels and that other changes since 1968 have had negligible effects. This value is based on average survival for the 1966-1968 period, before Lower Granite, Little Goose, and Lower Monumental (Table A.2.7-1).

H2:   Survival rates will not return to the same level as they were prior to the dams because of changes since the historical period, including increased shoreline development, the effects of introduced species, changes in upstream water regulation, and permanent changes in predator communities that result from impoundment of the river.

        The equilibrated survival rate under this hypothesis is 0.85, which is based on 1993-1996 average survival of wild juveniles for free-flowing reaches between Whitebird and Imnaha traps and Lower Granite (Table A.2.7-1).

Alternative hypotheses were implemented in FLUSH by "hardwiring" the survival rates within the code and bypassing model components related to survival through the four Snake River dams. In CRiSP, equilibrated survival rates were modeled by adjusting the predator densities upward (for the lower hypothesis of 0.85) or downward relative to the current value for that parameter.

Year	Wild/ Hatchery	Survival	Travel Time (Days)	Median Arrival Date	Reach	Downstream Project Survival	Mainly River Survival	km Mainly River	River Survival/km	River Survival/210 km
1966	Unknown	0.85	14	129.0	Whitebird (Salmon R.) - IHR Arrivals	0.950	0.895	339.00	0.999671955	0.9334
1967	Unknown	0.88	16	134.5	Whitebird (Salmon R.) - IHR Arrivals	0.950	0.926	339.00	0.999774244	0.9537
1968	Unknown	0.95	15	132.0	Whitebird (Salmon R.) - IHR Arrivals	0.950	1.000	339.00	1.000000000	1.0000
								Mean Historical Estimate	0.999815399	0.9624
1993	Wild	0.83	11.3	120.0	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.904	184.00	0.999453730	0.8916
1994	Wild	0.79	13	116.7	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.857	184.00	0.999158639	0.8380
1994	Wild	0.76	17.2	113.2	Imnaha Trap - LGR Tailrace	0.920	0.826	93.00	0.997947751	0.6496
1995	Wild	0.86	13.3	120.6	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.938	184.00	0.999652457	0.9296
1995	Wild	0.91	10.5	120.1	Imnaha Trap - LGR Tailrace	0.920	0.988	93.00	0.999870669	0.9732
1996	Wild	0.82	20.3	113.1	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.893	184.00	0.999388050	0.8794
1996	Wild	0.81	11.1	119.8	Imnaha Trap - LGR Tailrace	0.920	0.884	93.00	0.998671393	0.7564
						Recent Wild Mean:	Whitebird+Imnaha		0.999163241	0.8454
1993	Wild	0.83	9.4	123.1	Clearwater Trap - LGR Tailrace	0.920	0.898	12.00	0.991058639	0.1517
1994	Wild	0.84	13.1	116.5	Clearwater Trap - LGR Tailrace	0.920	0.915	12.00	0.992644381	0.2122
1995	Wild	0.88	12.1	111.2	Clearwater Trap - LGR Tailrace	0.920	0.955	12.00	0.996208143	0.4503
						Recent Wild Mean:	Clearwater		0.993303721	0.2714

A.3 Other Uncertainties / Alternative Hypotheses

This section is structured in a form parallel to Section 4.3 of the main report. It is worth reviewing Section 4.3 prior to reading this section.

A.3.1 Transportation Assumptions

Transportation Rules

The proportion of fish transported in the models prospectively is determined by the FGEs used in the passage models and the rules for spill and collection. The FGEs have been standardized amongst models and outlined in Section 4.2.2 and A.2.2. The rules for collection and spill for scenario A1 are as follows:

Case 1: If the seasonal average (April 10-June 20) flow is projected at <85 kcfs, implement as in Biop: no spill at LGR, LGS, LMN and transport all fish collected at those projects. No transportation at MCN.

Case 2: If seasonal average flow is projected to be >=85 kcfs and <100 kcfs, implement as in the Biop: no spill at LGR and transport all fish collected. At LGS and LMN spill to 12-hour levels described in Biop (80 and 81 % of total project flow, respectively) or to spill cap in 1997 Water Management Plan (50 and 40 kcfs, respectively) and transport all fish collected. No transportation at MCN.

Case 3: If seasonal average flow is >=100 kcfs, manage as in 1997: at LGR, LGO, and LMO spill to the 12-hour levels described in the Biop (80, 80, and 81 % of total project flow, respectively) or to the spill cap in the 1997 Water Management Plan (45, 50, and 40 kcfs, respectively). At LGR, transport all fish collected. At LGS and LMN bypass all fish from the "B" side of separators and transport all fish from the "A" side. No transportation at MCN.

In the passage models, average separator efficiencies for LGO and LMO were used for the prospective simulations.

For scenario A2, transport all fish collected at LGR,LGO,LMO, and MCN. There is no voluntary spill at collector projects.

For scenario A3, there is no transportation.

Transportation assumptions used with FLUSH (TRANS1 and TRANS2)

The transport (T) and control (C) data set used in the FLUSH transport models is summarized in Table A.3.1-1. The raw data represent all available transport studies conducted at Lower Granite (LGR) and Little Goose (LGO) dams, 1971-1989. The quantity T/C (Φ) is defined as follows:

$$T\!/\!C = \frac{\left[\dfrac{n_t}{N_t}\right]}{\left[\dfrac{n_c}{N_c}\right]} \qquad\qquad \text{[A.3.1-1]}$$

where:

n_t = number of marked adults returning to upper most transport project that were transported as smolts

N_t = number of marked smolts that were transported from the reference project

n_c = number of marked adults returning to upper most transport project that migrated in-river as smolts

N_c = number of marked smolts that migrated in-river

The T/C results from Snake River yearling chinook studies were used to develop a T/C function for use in the FLUSH passage model. Results from all studies were used, with the following exceptions: 1) Studies in which treatment fish were transported in saltwater; and 2) Study fish from 1968-70 were transported from Ice Harbor Dam (where mass transport has never been implemented); hence these studies were not included.

The yearly T/C estimates are the weighted geometric means of the T/C estimates derived from different release groups for that year. The T/C estimates come from the test and control release and recapture numbers in Park (1985), except source documents are used for years when Park presents only pooled numbers from multiple studies (1975 and 1976), and for transport studies done after Park's report (1986 and 1989). The source documents are also used for one freshwater transport release group study in 1978 which Park (1985) omits without explanation.

The T/C estimates are ratios of binomial proportions, with theoretical variances described by

$$\sigma_t^2 = \frac{p_t q_t}{N_t} \text{ and } \sigma_c^2 = \frac{p_c q_c}{N_c} \qquad\qquad \text{[A.3.1-2]}$$

for the transport recovery proportion (p_t) and control recovery proportion (p_c), respectively, with $q_t = 1 - p_t$ and $q_c = 1 - p_c$. The transport and control proportions are given by

$$p_t = \frac{n_t}{N_t} \text{ and } p_c = \frac{n_c}{N_c}, \qquad\qquad \text{[A.3.1-3]}$$

respectively. The variance of the natural log of the T/C estimates can be derived by using the delta method of variance approximation for a function of random variables. The resulting variance of the natural log of the T/C estimate is:

$$VAR\left(\ln\left(T\!\big/_{\!C}\right)\right) = \frac{1}{n_t} + \frac{1}{n_c} - \frac{1}{N_t} - \frac{1}{N_c} \qquad\qquad \text{[A.3.1-4]}$$

For years in which more than one useable study was performed, a weighted geometric mean T/C for each year was derived as follows:

Calculate the variance of the natural log of each usable T/C estimate for that year (see formula, above).

Calculate a weight for each study as the inverse of the variance:

$$W = \frac{1}{VAR\left(\ln T/C\right)}$$

Multiply the natural log of each T/C estimate by its corresponding weight:

Sum the weighted estimates over all useable transportation studies performed during a given year and calculate the weighted mean ln(T/C) by dividing the sum of the weighted estimates by the sum of the weights:

$$WeightedMean \ln\left(T/C\right) = \frac{\displaystyle\sum_{i=1}^{k} \frac{\ln\left(T/C\right)_i}{VAR_i\left(\ln\left(T/C\right)\right)}}{\displaystyle\sum_{n=1}^{k} \frac{1}{VAR_n\left(\ln\left(T/C\right)\right)}}$$

where there are "k" useable T/C ratios from studies performed within that year.

Calculate the weighted geometric average as the base of the natural logarithm raised to the power of the weighted mean ln(T/C):

$$WeightedGeometricMean\left(T/C\right) = \exp\left(WeightedMean \ln\left(T/C\right)\right)$$

In years where studies were done from both LGR and LGO, the data are combined without making any adjustment to the T/Cs, so the result is an estimate of the T/C for the LGR/LGO aggregate studies.

A primary data limitation is that controls are not fully representative of in-river migrants (IFWTS 1993; Mundy et al. 1994; Ward et al. in press). A key assumption of the transport studies is that the survival of the control fish, which were collected, handled and in most studies transported to their release site, accurately represents survival of in-river migrating fish and that their treatment during the test does not cause significant additional mortality above the level sustained by the in-river migrating population (IFWTS 1993). In an independent review of transportation research, Mundy et al. (1994) state:

> The use of the term, *control*, does not imply that the reviewers consider these individuals to actually be controls. The term is used only for consistency with the nomenclature used by the NMFS. The NMFS control group is considered by the review team to be another treatment group that is used for comparative purposes.

Reported T/Cs for some studies are inflated because control groups were transported upstream substantial distances and had to migrate through the upstream project twice. Controls were released 15-50 km upstream of the transport project in 1971, 1972, 1973, 1975, and 1976 (Table A.3.1-1). Therefore, smolt numbers for in-river controls were adjusted for the mortality of going through a dam twice in 1971, 1972, 1973, 1975,1976 and one group in 1978. Each set of parameter values for historic FGEs, bypass survival, turbine survival and spill efficiency (model runs R1-R8) required a separate adjustment of control fish. An adjusted T/C was then calculated for transport model 1 (Table A.3.1-2).

Table A.3.1-1:   Unadjusted T/C data, 1971-1989.

Year	Collection Point	Control Release Point*	Control Releases	Treatment Releases	Control Returns	Treatment Returns	T/C	Source
1971	LGO	LGO[a]	20673	30637	52	119	1.54	Park 1985
1971	LGO	LGO[a]	20673	35252	52	147	1.66	Park 1985
1972	LGO	LGO[a]	32836	54906	25	45	1.08	Park 1985
1972	LGO	LGO[a]	32836	51500	25	44	1.12	Park 1985
1973	LGO	LGO[a]	88170	83606	20	261	13.76	Park 1985
1973	LGO	LGO[a]	88170	57758	20	241	18.39	Park 1985
1975	LGR	LGR[a]	42915	30127	127	145	1.63	Park et al. 1979
1975	LGR	LGR[a]	42915	38423	127	294	2.59	Park et al. 1979
1976	LGO	LGO[a]	27315	36239	7	9	0.97	Park et al. 1980
1976	LGO	LGO[a]	12255	32366	1	16	6.06	Park et al. 1980
1976	LGR	LGR[a]	21711	47507	9	7	0.36	Park et al. 1980
1976	LGR	LGR[a]	2847	25411	1	9	1.01	Park et al. 1980
1978	LGO	LGO[b]	36441	49391	5	5	0.74	Park 1985
1978	LGR	LGR[c]	8249	56546	3	66	3.21	Park 1985, Park et al. 1983
1978	LGR	LGR[c]	8249	43855	3	33	2.07	Park 1985, Park et al. 1983
1978	LGR	LGR[c]	8249	38685	3	5	0.36	Park et al. 1982, 1983
1979	LGR	LGR[c]	25532	27336	3	12	3.74	Park 1985
1986	LGR	LGO[d]	45035	45004	47	74	1.58	Matthews et al. 1992
1989	LGR	LGO[d]	107176	75295	28	46	2.34	Harmon et al. 1993

* Ward et al. 1997:

[a] Trucked 15-50 km upstream from   collection site

[b] Released directly into tailrace of dam where collected

[c] Trucked downstream and released into tailrace of dam where collected

[d] Trucked downstream to tailrace of LGO, though collected at LGR

Table A.3.1-2 below contains the annual weighted T/C ratios used to construct a function which predicts T/C ratios from in-river survival. There is a pair of different parameter values for each assumption for historic dam passage survival.   In the prospective analysis, the FLUSH reservoir survival/FTT relationship calibration is matched with the T/C function for the same TURB assumption.

Table A.3.1-2: Annual Weighted geometric mean Transport to Control ratios (T/C) for Snake River yearling chinook. The adjusted T/Cs are computed by adjusting the number of control fish for the mortality experienced by being transported upstream of the collector project and migrating through that project twice. Mortality adjustment is based on TURB assumption from passage models.

Year	T/C 1/	T/C 2/	T/C 3/	T/C 4/	T/C 5/
1971	1.60	1.54	1.54	1.52	1.54
1972	1.10	0.95	0.78	0.97	0.93
1973	15.90	14.51	8.43	13.04	14.31
1975	2.12	2.04	1.90	2.01	2.03
1976	0.77	0.73	0.68	0.72	0.73
1978	1.30	1.27	1.14	1.25	1.27
1979	3.74	3.74	3.74	3.74	3.74
1986	1.58	1.58	1.58	1.58	1.58
1989	2.34	2.34	2.34	2.34	2.34

1/ unadjusted geometric mean T/C
2/ adjusted geometric mean T/C (controls adjusted for TURB 1 assumptions)
3/ adjusted geometric mean T/C (controls adjusted for TURB 4 assumptions)
4/ adjusted geometric mean T/C (controls adjusted for TURB 5 assumptions)
5/ adjusted geometric mean T/C (controls adjusted for TURB 6 assumptions)

Controls were collected, handled and marked at the study project, unlike true in-river migrants which experience a combination of collection/bypass, turbine and spill passage routes at this site. Preliminary analysis of return rates of PIT tagged wild smolts from 1994 to 1995 further suggests that delayed mortality of in-river migrants may be related to route of passage through the hydropower system (Weber et al. 1997). Smolts that were detected (i.e., were bypassed) two or more times returned at lower rates than those detected one or zero times (Figure A.3.1-1). The T/C data were not adjusted in either transport model to account for these potential problems.
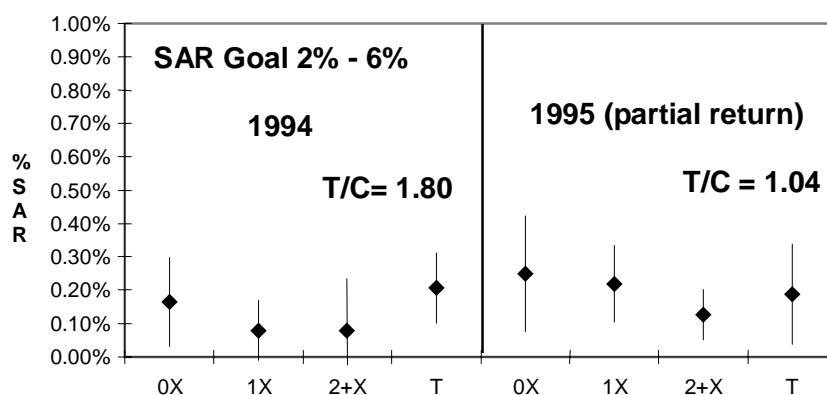
Figure A.3.1-3: Smolt to adult return rates of wild yearling PIT tagged chinook released above lower Granite Dam, 1994-95 (NMFS data). 0X, 1X and 2+X refer to number of times a smolt was detected and bypassed, T refers to transported smolts.

Two transport releases were made in 1975 (Table A.3.1-1), one group was branded and coded-wire tagged and the other was wire-tagged only, to determine whether the additional stress of double marks

had a negative effect on survival (IFWTS 1993). Controls in 1975 were double marked with wire-tags and brands. Park et al. (1979) reported that the wire-tagged only transport group had significantly higher T/C than the double marked group (2.60 vs. 1.63; Table A.3.1-1). IFWTS (1993) stated that the more appropriate comparison in 1975 was the reported T/C of 1.63 because of the marking differences. Both reported T/C values were included in the data set for the fitted transport models.

Available T/C estimates do not span the full range of recent passage conditions. No controls and few transported chinook returned from the worst flow year, 1977 (Park 1985; Mundy et al. 1994). Also, transportation studies were not conducted during years with higher flows and spills (e.g., 1982-1984 smolt migrations) in which Snake River chinook stocks experienced overall higher smolt-to-adult return rates (Raymond 1988) and adult-to-adult survival rates (Deriso et al. 1996; Schaller et al. 1996).

The ability of transportation to improve survival to the spawning grounds and hatcheries has not been demonstrated for spring/summer chinook because transport studies were conducted at the mainstem dams (Mundy et al. 1994). Some evidence suggests that T/Cs are lower in natal areas than at the dams (Olney, et al. 1992). Based on recoveries at Lower Granite Dam and natal areas (hatcheries and spawning grounds) from the 1986 and 1989 transport studies, the estimated T/C in the natal areas was 83% of that estimated at the mainstem dams. This pattern was explored in transport model 2 which used a factor of 0.83 to adjust the T/C estimates of transport model 1.

The 1971 through 1979 survival study estimates were expanded to estimate in-river survival below the transport project to Bonneville Dam. Since there were no reach survival studies in 1986 and 1989 estimates were generated through FLUSH. The adjusted T/C (Table A.3.2-2) and in-river survival estimates from Lower Granite (LGR) and Little Goose (LGO) dams on the Snake River were used in FLUSH.

Transport models were fit to the following form:

$$T/C = (1/(1 + \exp(-(s - a) / b))) / s \qquad [A.3.1\text{-}5]$$

where:

$s =$ in-river survival from tailrace of the transport dam; and
$T/C =$ adjusted T/C ratio.

The transport rules described above were used to estimate the proportion of the population of smolts that are transported in the prospective simulations alternative management scenarios. The T/C estimates are used in the prospective passage model simulations to calculate system survivals. The retrospective and prospective λ_n's, along with the retrospective and prospective system survivals are then used in equation A.3.2-20 to derive the prospective river mortality rate (m_y) to use in prospective BSM runs. The annual T/C values are generated in the passage models using the function in equation A.3.1-5 which relates T/C to in-river survival (V_n). Note that the D values (the ratio of delayed survival factors for transported fish to those of in-river migrating fish) are computed by equation A.3.2-13, and are a consequence of the passage model projected V_n and T/C. The important point to note, therefore, is that D values are specific to each passage model and are not an empirical measurement.

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Comment on Transportation assumptions in FLUSH by Williams and response by Schaller et al.:

There is insufficient justification to adjust the transport T/Cs by 0.83, particularly if it is based on conclusions from transport reviews in the early 1990s which only looked at results from 1986 studies. Conclusions are meaningless because of small sample sizes and results from other years, particularly recent ones do not show the same trends.

Response by Schaller et al.: This was one of two models in a sensitivity; the other assumed no decrease in T/C from the dam to the natal areas. Based on available information, one cannot conclude that T/C ratios do not decrease between the upper dam and the natal areas, therefore the sensitivity is justified. This sensitivity used the geometric mean of two years studies (1986 and 1989):

		Smolts	*Adults*	*Adults*	*T/C*	*T/C*	*natal/dam*
Year	*Group*	*released*	*at dam*	*at natal*	*dam*	*natal*	*ratio*
1986	*C*	*45035*	*47*	*28*			
1986	*T*	*45004*	*74*	*29*	*1.58*	*1.04*	*0.66*
1989	*C*	*107176*	*28*	*14*			
1989	*T*	*75295*	*46*	*24*	*2.34*	*2.44*	*1.04*

Geometric mean = 0.83

End of comment on transportation assumptions in FLUSH by Williams and response by Schaller et al.

Comment on transportation assumptions in FLUSH by Anderson and response by Wilson et al.:

The critique of the FLUSH transportation assumptions is developed from an initial Critique (Anderson December 22 1997) and a response (Wilson et al. January 9 1998). The FLUSH transport hypothesis "... does not single out one particular mechanism or index among many to predict direct or delayed mortality" (Wilson 1998). It assumes that in-river survival V_s is a measure of the factors affecting both transport and non-transport fish returns for both retrospective and prospective years. The empirical relationship, which is, they admit, not mechanistic, is written

[15]

where T/C are adjusted, a and b are regression constants, and V_s is the "adjusted" in-river survival from the transport dam to Bonneville Dam tailrace. The adjusted in-river survival is expressed by

[16]

where V_t is survival to the transport dam and V_n is the total in-river survival.
Derivation of the FLUSH hypothesis for D follows from Eq [15] as

$$[17]$$

where for simplicity the coefficients $\alpha = \exp(a/b)$ and $\beta = 1/b$ are defined and these constants are extracted from the regression of T/C to V_s. according to Eq [15], and V_T is the direct transport mortality and is generally assumed constant at ~ 0.98. It then follows that D is only a condition of the adjusted in-river survival V_s.

Equations [15], [16] and [17] become a foundation of the delayed mortality hypothesis in FLUSH. The model was derived from retrospective data between T/C and V_s and the same relationship is used to predict prospective T/C from prospective V_s and consequently prospective D values. There are two problems with this approach, one statistical and one conceptual.

The statistical problem involves whether or not Eq [15] is significant. The regression used data from 1971 through 1989 and excluded T/C studies from 1968 to 1970 and the more recent studies in 1994 and 1995. In addition the individual studies within years were averaged so different release and transport conditions were averaged. To gain some idea of the statistical significance Eq [15] was rearranged in a linear form to estimate the regression coefficients α and β. Putting Eq [15] in the form

$$[18]$$

the regression results using TURB4 adjusted T/C and V_s were:

> Residual Standard Error = 0.2517,   Multiple R-Square = 0.5026
> N = 9, F-statistic = 7.074 on 1 and 7 df, p-value = 0.0325
>   coef   std.err t.stat   p.value
> $\ln \alpha$ -0.2401 0.0272 8.8407 0.00001
> β 0.0738 0.0277 -2.6597 0.0325

The regression was significant, but it was confounded by having V_s in both the dependent and independent terms. To assess the actual significance of the regression the STFA (state and tribal fisheries agencies) need to present the statistical details of the regression used to estimate Eq [15].

Response by Wilson et al.:The function was fit by non-linear routine, so the independent variable (Vs) did not appear in the dependent part of the equation.

Comment by Anderson continues:

The second problem with the FLUSH delayed mortality hypothesis is the invariant coupling of transport and non-transport delayed mortality with in-river survival. This is best understood from Eq [16] although it is also contained in the T/C definition through Eq [15]. The parameter D is the ratio of mortalities of two groups of fish that have vastly different histories prior to entering the estuary. Expressing their ratio through a single variable, the in-river survival of the non-transport group, is a strong inference that is only achieved in two possible conditions. Either the delayed

mortality in one group is constant over all years or there is an explicit relationship between the two groups that is invariant with changes to the hydrosystem or transportation. The STFA claim the latter but since the hypothesis is not mechanistic there is no explanation of its basis.

In particular the hypothesis requires that λ_T will change for any change in V_s irrespective of the transported fish experiencing any of the in-river effect. That is, delayed transport survival will change if changes are made on the hydrosystem downstream of the transported fish collection point. To demonstrate this characteristic we write the incremental change in λ_T for an incremental change in V_s. The equation is

[19]

where all terms are positive. From the regression of Eq [18] $\alpha = 1.27$, $\beta = 0.0738$, $V_T \sim 1$ and from the STFA hypothesis improvements in non-transported fish in-river survival will improve their delayed post-hydrosystem survival. Under these conditions Eq [19] is positive so changes in non-transport fish survival directly changes the delayed survival of transport fish. This result is problematic it is difficult to envision a biological mechanism that would produce this effect. Note that by considering the derivatives as is done in Eq [19] the ambiguities of dealing with the ratio of survivals in Eq [17] are clarified. [Note: Hinrichsen pointed out this result can be simply demonstrated by substituting Eq. [20] into Eq.[17] and arranging the expression in terms of λ_T .]

In the CRiSP descaling hypothesis, x affects transport delayed mortality according to Eq [14] and in-river survival according to Eq [5]. Changes in V_{res} or N do not affect descaling and do not affect delayed transport survival. In the FLUSH hypothesis, changes in any variable that affects in-river survival also affects D. This results in a problematic characteristic in the FLUSH hypothesis. It says that all experiences of the non-transported fish below the collection dam directly affect the fate of the transported fish in the estuary and ocean, even though the transport fish do not pass through the river. For example, under the FLUSH hypothesis removal of McNary Dam would improve the post hydrosystem survival of fish transported from Lower Granite by truck or barge.

Response by Wilson et al. The problem with this conclusion is that this CRiSP approach was not used in the analysis in the Decision Analysis report. However, more important for the approach relating descaling to D is how the T/C experiments were performed. Mundy et al. 1994 point out that "only those fish judged to be in good condition are typically diverted to the marking stations; fish that are diseased, descaled, previously marked, or in poor condition are systematically excluded from the experimental lots (Smith et al. 1981, Matthews et al. 1988)." Note that the D values are estimates specific to each passage model and are not an empirical measurement. It is difficult to follow the logic of the relationship of descaling to D, given the fact that the descaled fish were excluded from the transport experiments which he uses to fit his relationship.

End of comment on transportation assumptions in FLUSH by Anderson and response by Wilson et al.

Transportation assumptions used with CRiSP (TRANS 3)

Transportation effectiveness is characterized primarily by the ratio of the post Bonneville survival of fish that were transported to Bonneville Dam to fish that arrive in the tailrace via an in-river passage route. The relative effect is expressed by D which is defined as:

$$D = \lambda_t/\lambda_n \hspace{4cm} \text{[Eq. A.3.1-6]}$$

where λ_t is the post Bonneville survival of transported fish and λ_n is the post Bonneville survival of non-transported fish.

On theoretical grounds, D is expected to change with differences in the ability of transported and non transported fish to survive in the post hydropower system life stage. For convenience, we refer to this as the estuary passage stage.

A primary factor determining how D will change over time is the relative condition of transport and non-transport fish as they begin their passage through the estuary. The working hypothesis is that fish condition affects fish survival. Fish condition of the two groups can depend on a number of factors including, but not limited to, stress in collection including descaling and routing into barges, stress in transportation including stress of mixing species in barges and trucks, overcrowding in barges and trucks, and stress at release back into the river below Bonneville Dam. These factors all may have yearly variations resulting changes in hydrosystem and transport operations. In addition fish stress can vary from year to year because of natural variations in environmental conditions prior to and during transportation and in-river migration.

In general we expect D to have changed from year to year as reflected by natural variations in the river system and by improvements in hydrosystem and transportation operations that have reduced fish stress. Perhaps the major improvement to operations was the implementation of a trash removal program in 1980. As was noted by Williams and Matthews (1995), prior to the regular removal of trash, the percent of both transported and bypassed fish with observed signs of descaling was significant and variable. After the inception of the trash removal program descaling declined. This change in the descaling levels significantly improved the condition of fish released from barges. A second stressor that has been reduced over time is due to changes in the mode of transportation. In early years of the transportation program fish were trucked as well as barged, while in the current system fish are barged. Evidence suggests that fish were abnormally stressed during trucking and so the current transport program should be releasing more vital fish below the hydrosystem.

Although these improvement have contributed to increased transport fish survival, in general fish that have passed in-river may, as a group, be stronger because the weaker members were more likely to have been culled from the population than the weaker members of a transport group. In transportation, this culling effect occurs in the estuary, not the hydrosystem, so part of the difference in the survival of transport and non-transport fish in the estuary is a reflection in the differences in culling through the two routes. This initial, or pre-transport, fish vitality distribution is expected to vary from year to year according to environmental conditions prior to and during the early stages of juvenile migration. Under this hypotheses the difference in the vitality of transport and non-transported fish below the hydrosystem will depend on the initial distribution of weak and strong fish at the top of the hydrosystem.

The estimation of D requires information on the in-river survival of non-transported control fish, Vn, an estimate of the barging survival Vt, and the transport to control ratio TCR. In years where a portion of the control fish are transported at a lower dam, D also requires the estimation of the fraction of the control fish that were transported, f.

Applying estimates of these parameters (V_n from CRiSP, V_t of 0.98, and TCRs from Table A.3.1-1) over the years of transport experiments (1968-1995) the D value exhibits an upward trend with high variability prior to 1980 and a more stable distribution after 1980. For retrospective analyses, it was assumed that the D value prior to 1980 was 0.5 and the D value after 1980 was 0.85 (see Table 4.3-2). This indicates that in the early years of the transportation program transport fish experienced more post-Bonneville mortality than in-river passing fish. In the recent period, the differences in survival of the two groups is reduced and is likely to reflect the effect of differences in where weaker fish are culled. The increase in D in more recent years is believed to be related to a reduction in descaling (Figure 4.3.1-2). For prospective analyses, D was selected randomly from the set of post-1980 D values.

Figure A.3.1-2. Percent descaling at transport dams (LGR or LGS).

Very recent work by James Anderson has developed a new hypothesis in which D is related to descaling estimates, and different hypothesis in which D is related to descaling estimates, and different release times are used to estimate the survival of control fish in T:C studies (i.e., V_n). This results in post-1980 D values of 0.6 to 0.7, rather than around 0.85. However, the results presented in this report used the higher D values that average to 0.85 after 1980.

Comment on transportation assumptions in CRiSP:

> Why were the values used in the retrospective analyses (0.5 before 1980, 0.85 after 1980) different from the mean values for these periods as shown in Table 4.3-2?

End of comment on transportation assumptions in CRiSP.

General comments on Section A.3.1 (Transportation assumptions) by Williams:

> Transportation studies that used coded-wire tagged (CWT) and branded fish for evaluation were conducted between 1968 and 1989 at Ice Harbor, Little Goose, and Lower Granite Dams. A complete set of the data of estimated in-river survivals through the hydropower system for each year compared to an inverse of the transport to control ratio is presented in the upper figure below. In-river survivals were estimated from past studies conducted during the years of transportation or from more recent studies and adjusted for estimated conditions in the past. Data

is plotted from the uppermost dam where fish were transported in years when more than one dam were used.

The earliest studies were conducted with fish placed into trucks and released at sites below Bonneville Dam. When the initial research studies indicated that more adult fish returned from juveniles that were transported around the hydropower system than from those that migrated through the hydropower system, a decision was made to maximize the number of fish collected and transported from upper dams on the Snake River. Barges were constructed to haul the fish because the capacity of trucks was too low. Barges are now used to transport nearly all spring migrants; therefore, research studies that were conducted in the past to evaluate transportation that utilized trucks as a means to haul fish to below Bonneville Dam do not represent the present operational mode. The lower graph below presents the data from evaluations where only barged fish were used as a means of transportation.

If concerns exist about the usefulness of data in years when control fish were hauled by truck to below Little Goose Dam from Lower Granite Dam, then only the 1978 and 1979 studies are usable. However, in these 2 years, many of the in-river controls that were released at Lower Granite Dam were collected and transported from Little Goose Dam.

In summary, NMFS does not believe any of the studies where transported fish were trucked to below Bonneville Dam represent conditions that will occur in the future and are therefore usable in prospective modeling.   Only studies in 1978, 1979, 1986, and 1989 have complete returns of usable data. Preliminary returns for 1995 studies do not include 3-ocean fish, although likely 80% of the hatchery fish have returned.
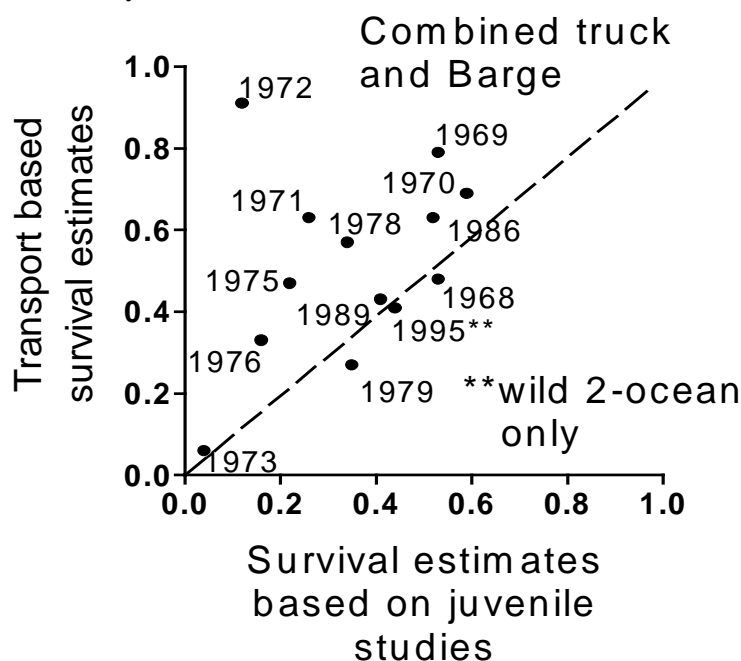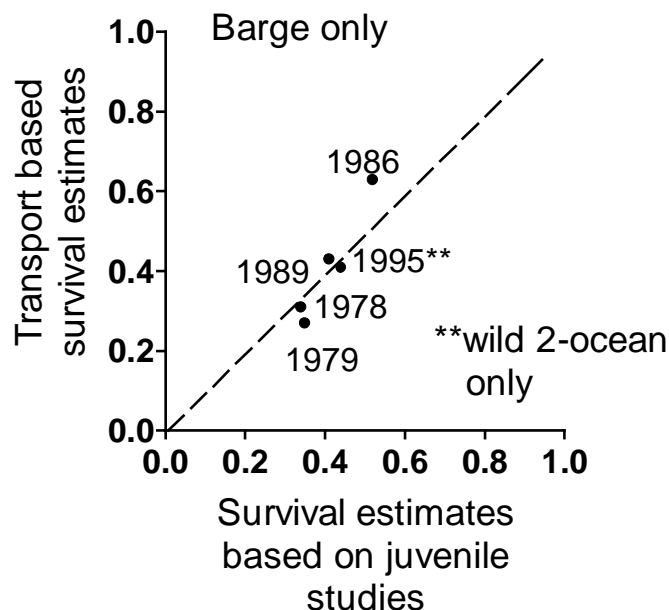
End of general comments on Section A.3.1 (Transportation assumptions) by Williams.

A.3.2 Stock Productivity

For prospective modeling, we used two alternative representations of chinook population dynamics.

Delta Model

The Delta model is described in the Aug. 1 memo by Wilson et al. (1997) titled "Draft General Framework for Prospective Modeling, with one proposed hypothesis on delayed mortality". The basic equation is:

$$\ln(R_{t,i}) = (1+p)\ln(S_{t,i}) + a_i - b_i S_{t,i} - M_{t,i} - \Delta m_{t,i} + \delta_t + \varepsilon_{t,i} \qquad \text{[Eq. A.3.2-1]}$$

in which we can write total passage + extra mortality rate *m* as

$$m = M + \Delta m \qquad \text{[Eq. A.3.2-2]}$$

for which,

$R_{t,i}$	=	Columbia River "observed" returns (recruitment) originating from spawning in year t and river sub-basin i
$S_{t,i}$	=	"observed" spawning in year t and river sub-basin i
a_i	=	Ricker *a* parameter, which depends on sub-basin i
b_i	=	Ricker b parameter, which depends on sub-basin i
p	=	depensation parameter
$M_{t,i}$	=	direct passage mortality which depends on year and region
$\Delta m_{t,i}$	=	extra mortality rate, which depends on year and region
δ_t	=	year – effect parameter for year t

$\varepsilon_{t,i}$ = normally distributed mixed process error and recruitment measurement, which depends on year t and sub-basin i.

$N_{o,n}$ = number of smolts at the top of first reservoir that are destined not to be transported

V_t - weighted direct passage survival of all transported smolts

A recommended approach for segregating survival in the prospective model uses system survival (ω), which is the output from passage models used in the past for linking passage and life-cycle models. System survival is the number of in-river equivalent smolts below BON divided by the population at the head of the first reservoir. The numbers of transported smolts at each collector project that survive to BON are converted into in-river equivalents by adjusting for differential delayed mortality using the transport/control ratio (Φ) and in-river survival (V_n). System survival is then

$$\omega = P_{0,n}V_n + \Phi V_n(1 - P_{0,n}) = V_n\left[P_{0,n} + \Phi\left(1 - P_{0,n}\right)\right] \qquad \text{[Eq. A.3.2-3]}$$

Terms and Derivations

Smolts can pass the hydrosystem by one of five routes (subscripts 1,2,3,5, n). The numbers represent dams where collection takes place, in order from the top of the reservoir: 1 = LGR, 2 = LGO, 3 = LMN, 5 = MCN. The subscript 'n' represents smolts which are never transported, i.e., smolts which migrate in-river through the entire hydrosystem.

i = region
j = passage route
t = transported
n = non-transported
b = at Bonneville tailrace

Note: All variables described afterward in this section refer to annual seasonal values: the 'y' subscript is omitted for simplicity.
The following variables are estimated in the passage models:

N_0 = total number of smolts at top of first reservoir in a season
N_j = number of smolts reaching the forebay of dam j
N_b = total number of smolts alive at Bonneville during a season
L_j = cumulative in-river survival from top of first reservoir to dam j
$N_{j,t}$ = number of smolts collected for transportation from dam j
V_n = $N_{b,n} / N_{0,n}$ = direct passage survival of smolts passing in-river
V_j = L_j * bypasssurv * bargesurv = direct passage survival of smolts transported at dam j

$$V_T = \sum_{j=1}^{5} V_j \frac{P_{0,j}}{P_{0,T}} \qquad \text{[Eq. A.3.2-4]}$$

where
$P_{0,j}$ = proportion of smolts at head of first reservoir destined to pass by route j and

$$P_{0,T} = \sum_{j=1}^{5} P_{0,j}$$

and

$P_{0,T}$ = proportion of smolts at head of first reservoir destined to be transported at any project.

Estimates of values for P's, L, and V's come from a passage model.

Derivation of $P_{0,j}$'s:

$$P_{0,j} = P_j \qquad \text{if } j = 1$$

$$P_{0,j} = P_j \cdot \prod_{k=1}^{j-1}\left(1-P_k\right) \quad \text{if } j > 1, \neq n$$

where P_j is the fraction of smolts arriving at dam j that are transported.

If $j = n$, then $P_{0,j}$ = proportion of smolts destined to pass in-river = $P_{0,n}$
and

$$P_{0,n} = 1 - \sum_{j=1}^{5} P_{0,j} \qquad\qquad \text{[Eq. A.3.2-5]}$$

System survival can also be expressed as follows:

$$\omega = \exp[-M][DP + 1 - P] \qquad\qquad \text{[Eq. A.3.2-6]}$$

where the P is the fraction of smolts immediately below Bonneville which were transported. The D is the ratio of post-Bonneville survival factors of transported to non-transported smolts (see equation 4.3.1-1). M is direct passage river mortality over a season and is estimated in the passage models as follows:

$$M = -\ln\left(V_n P_{0,n} + \sum_{j=1}^{5} V_j P_{0,j}\right) \qquad\qquad \text{[Eq. A.3.2-7]}$$

or can be estimated in the passage models by:

$$M = -\ln(N_b/N_0) \qquad\qquad \text{[Eq. A.3.2-8]}$$

The delayed mortality component, given a particular passage model, can be estimated by setting it equal to the MLE estimate of direct and delayed passage mortality (*m* in Chapter 5) minus an estimate of direct passage mortality (*M*, above).

Then,

$$\Delta m = m - M, \quad \text{with } m = M_d + \mu. \qquad\qquad \text{[Eq. A.3.2-9]}$$

where M_d is the direct passage mortality for downriver stocks and μ is net (direct + delayed) instantaneous mortality from the Snake River subbasins to the John Day Dam. Annual estimates of M and M_d would be provided by survival estimates from the passage models without any delayed mortality applied. For downriver stocks, μ by definition is equal to zero; therefore for downriver stocks $m = M (= M_d)$, so $\Delta m = 0$.

The delayed mortality factor (Δm) can be expressed as a delayed survival factor as follows:

$$\exp[-\Delta m] = \lambda_n[DP + 1 - P] \qquad\qquad \text{[Eq. A.3.2-10]}$$

The system survival (ω) is equal to the ratio of exp(-m) to the post-Bonneville delayed survival factor of in-river fish. That is:

$$\omega = \frac{\exp(-m)}{\lambda_n}$$
[Eq. A.3.2-11]

The delayed mortality associated with in-river fish, given a particular passage model, can be estimated by setting it equal to the MLE estimate of direct and delayed passage mortality (m in Chapter 5) minus an estimate of -ln(ω) (the instantaneous system mortality). This difference is -ln(λ_n), which is the delayed instantaneous mortality of in-river equivalent fish. The post-Bonneville survival factor for non-transported smolts (λ_n as in equation A.3.2-10), is derived by rearranging equation A.3.2-11:

$$\lambda_n = \frac{\exp(-m)}{\omega}$$
[Eq. A.3.2-12]

The D is the ratio of post-Bonneville survival factors of transported to non-transported smolts and can be expressed as follows:

$$D = \frac{\lambda_T}{\lambda_n} = \Phi \frac{V_n}{V_T}$$
[Eq. A.3.2-13]

The parameter V_T is direct passage survival of smolts transported at all dams and is estimated through the passage models. The Φ is the quantity referred to as the transport to control ratio (T/C). The T/C ratio is a relative measure of return rates for migrating smolts which have been trucked or barged around the hydrosystem to those smolts which migrated through the hydrosystem. The T/C ratios have been estimated for fish being transported from Snake River dams through mark recapture studies. The T/C values have been estimated in the passage models through a function which relates T/C to in-river survival (V_n). Note that the D values are computed by equation A.3.2-13, and are a consequence of the projected V_n and T/C. The values for V_T and V_n are estimated through the passage models. The T/C values can be estimated by a T/C function as described in Section A.3.1. An alternative prospective method is to plug in T/C values for years data are available into the passage models and then generate D values retrospectively. The resulting retrospective values of D can be randomly sampled to generate T/C values in prospective simulations. This would provide system survivals which can be used in the BSM. Therefore, the important point to note is that D values are specific to each passage model and are not empirical measurements.

Delta Model Sensitivities

The prospective modeling using the delta model relies on the posterior distribution of its parameters, obtained by sampling the likelihood function described in Deriso et al. (1996). The maximum likelihood estimates were tested for sensitivity to the exclusion of various subsets of observations: stocks, brood years, and various other subsets of data, including individual points (Hinrichsen 1998). Differential mortality, dam mortality, and intrinsic productivity estimates were each sensitive to the exclusion of the John Day Middle Fork stock, especially the early part of the record (1959-1973), where three data points (1959, 1964, and 1968) were driving the high influence. These were the three most influential observations in the entire data set. Exclusion of the John Day Middle fork data decreased the differential mortality (μ) and X-dam mortality estimates and the intrinsic productivity estimates (Ricker -as) of the Snake River stocks. When the John Day Middle Fork was omitted, (1) the average estimate of mu (1970-

1990) fell from 1.455 to 0.840, (2) the average Snake Ricker-a fell from 3.13 to 2.19 and (3) the X-dam survival rate increased from 0.77 to 0.92 (per dam mortality decreased from 0.26 to 0.08). These represent large shifts in the mean of the parameter distributions. For example, the mean of the dam mortality distribution shifted to 0.08, which was the distribution's 12th percentile when the John Day Middle Fork data were included. Interestingly, the average Snake Ricker-a with the stocks deleted (2.19) was near the average lower river Ricker-a (2.28).

Deletion of data by brood year showed some large changes in the opposite direction. Deletion of brood year 1963, for example, increased mu from 1.455 to 1.675. However, there is no single stock is driving this large increase and, to our knowledge, there is no reason for deleting this brood year based on data quality. It may be possible to estimate a separate X-dam mortality for the year 1963 to reduce its influence, but we have no basis, aside from its high influence, for treating 1963 different than other years.

The early John Day Middle Fork spawner recruit data are of questionable quality and are not representative of the bulk of the lower river spawner-recruit data. Difficulties with using the Middle Fork as a control for the Snake stocks are many. They include: (1) The Middle Fork was treated with rotenone and other chemicals harmful to salmon in 1966, 1973, and 1974. (2) Prior to 1976 a dam, mill and town were located at Bates (river kilometer 107), creating a barrier to salmon migration on the Middle Fork. (3) The index redd counts were not a consistent indicator of spawning in index streams in a 1978-1985 study (Lindsay et al. 1986). One concern we have is that we are not fully aware of the quality of the John Day Middle Fork relative to the other stocks. Another concern is that this stock was scrutinized because of its high influence. Other observations may also have high influence in an opposite direction and similar data quality problems. Another concern we have about conclusions regarding the quality of John Day Middle Fork data is that this particular set of data was examined in more detail only after it was discovered that the MLE estimates were particularly sensitive to three years of that data set.

Further study is needed to determine the extent of the influence of these observations on the prospective results.

References

Deriso, R., D. Marmorek, and I. Parnell. 1996. Retrospective analysis of passage mortality of spring chinook of the Columbia River. Chapter 5 of PATH report "Plan for Analyzing and Testing Hypotheses: Draft Final Report on Retrospective Analyses," July 29, 1996.

Hinrichsen, R. A. 1998. Influence of Exceptional Spawner-Recruit data of the John Day Middle Fork on the Delta Model Parameters. PATH memo. See http://www.cqs.washington.edu/~hinrich/PATH/INFLUENCE/abst_inf.html

Lindsay. R.B., W.J. Knox, M.W. Flesher, B.J. Smith, E.A. Olsen, and L.S. Lutz. 1986. Study of Wild Spring Chinook Salmon in the John Day river System. Final Report 1985. Bonneville Power Administration, Portland, Oregon.

Alpha Model

The alpha model is described in Hinrichsen and Anderson, September 4,1997 memo "Passage and prospective model linkage (alpha model)"). The basic equation for the Alpha model is:

$$\ln(R_{t,i}) = (1 + p)\ln(S_{t,i}) + a_i - b_i S_{t,i} - M_{t,i} - \alpha_{t,j} + \varepsilon_{t,i} \qquad \text{[Eq. A.3.2-14]}$$

The alpha term can be written as

$$\alpha = \alpha_n - \overline{\alpha}_n - \ln(DP + 1 - P) + \overline{\ln(DP + 1 - P)} \qquad \text{[Eq. A.3.2-15]}$$

where

$$\alpha_n = c_1 / F + c_2 E / F + c_3 E^2 / F + STEP \qquad \text{[Eq. A.3.2-16]}$$

as defined in Equation (14) of Anderson and Hinrichsen (1997), except that we've dropped the quadratic term due to some difficulties we've discovered. Note that STEP is a step function which is 0 for brood years 1952-1974.

<<Need to define the period over which the two averages are computed in [Eq. A.3.2-15]>>

Representation of Stock Productivity in the Delta and Alpha models

A number of relationships can be written between parameters of the delta model and alpha model, including

$$\exp[-\Delta m] = \lambda_n [DP + 1 - P] \qquad \text{[Eq. A.3.2-17]}$$

$$\omega = \exp[-M][DP + 1 - P] \qquad \text{[Eq. A.3.2-18]}$$

$$\exp[-m] = \omega \lambda_n \qquad \text{[Eq. A.3.2-19]}$$

in which, ω is system survival and λ_n is post-Bonneville survival factor for non-transported smolts, as defined in Wilson *et al*, and in which D is the ratio of post-Bonneville survival factors of transported to non-transported smolts and P is the fraction of smolts at Bonneville which were transported, as in Hinrichsen and Anderson's paper.

The last equation given above can be used to write an equation for total passage + extra mortality during any prospective year *y* in terms which involve its coupled retrospective water year *r*:

$$m_y = m_r - \ln[\frac{\omega_y \lambda_{n,y}}{\omega_r \lambda_{n,r}}] \qquad \text{[Eq. A.3.2-20]}$$

The hypotheses described in Section A.3.3.3 allow us to collect terms in the delta model and write directly comparable quantities in the alpha model. The idea is to write the models where components that differ in the prospective analysis can be identified.

With the definitions below, both the delta and alpha model can be written as a Ricker model with 4 specially defined terms,

$$ln(R) = (1+p)ln(S) + \textbf{Term1} + \textbf{Term2} + \textbf{Term3} + \textbf{Term4} \text{ -}bS + \varepsilon \qquad \text{[Eq. A.3.2-21]}$$

The hypotheses in Section A.3.3.3 revolve around the interpretation of **Term3,** that is $\ln(\lambda_n)$. The passage models provide the same standard input **Term2**, that is ln(ω), to each of the models. The other

terms are in a sense particular ways chosen to estimate intrinsic productivity and common climate variables (common in that they affect equally both transported and non-transported smolts).

Term1: Ricker "*a*" value. Collect all constant terms in each model other than those needed to center climate variables modeled as Markov processes.

> In the delta model, this is the term "a"

> In the alpha model, this is the term "a - average[ln(DP+1-P)] +average [STEP]

Term2: logarithm of system survival ,ln(ω)

> In both models, this is the term "-M +ln(DP +1-P)"

Term3: logarithm of post-Bonneville survival factor of non-transported fish $\ln(\lambda_n)$

> In the delta model, this is the term "-m- ln(ω)"

> In the alpha model, this is the term "-STEP"

Term 4: Markov type climate variables, centered so they sum to zero over the brood years 1951-1990

> In the delta model, this is the term δ_t

> In the alpha model, this is the term, "c_1 (1/F- average(1/F)) + c_2(E/F - average(E/F))"

A comparison of estimates of the Term1 (adjusted Ricker "*a*" values) are shown in Figures A.3.2-1 to A.3.2-3 (Figs. 17-19 from R. Deriso's October paper; not available for this draft). The adjustments on Figure A.3.2-2 and A.3.2-3 for "extra mortality" of non-transported smolts provide a meaningful way to compare productivity measures for the alpha and delta models.

Comment on Section A.3.2 (Stock Productivity) by Williams and response by Schaller et al.:

> Snake River and Columbia River stock groupings represent fish from two distinct species as defined under the Endangered Species Act and come from two different Evolutionarily Significant Units (ESUs). Therefore, fish from these two ESUs may have systematic differences in survivals that change independent of each other under varying environmental (both physical and biological) conditions. Though some of these differences are accounted for by different Ricker a and b parameters, such stock differences may confound the interpretation of *m* in the Delta model, which is partly derived from upstream-downstream differences in (R/S)). Does it represent hydrosystem effects, or inherent stock differences in the response to changing ocean conditions? The Alpha model, on the other hand, assumes that all extra mortality for the two stock groupings is independent (i.e., no common year effects). Altogether, the combination of the Alpha/Delta models and the alternative hypotheses for extra mortality (i.e., hydrosystem dependent, regime shift, or BKD / mortality here to stay) account for a wide range of possible interpretations of historical stock-recruitment patterns.

> Intrinsic stock productivities may change over time. A recent report by Kiefer, et al. (1997) to the Bonneville Power Administration provided information that egg to smolt survivals from the upper Salmon River from 1988 through 1994 averaged only 2.2%. If levels were this low in the 1960s,

it probably would have led toward a quick crash of the stocks. Thus, it seems likely that it is a recent phenomenon and may relate to small spawner sizes in successive years.

Response by Schaller et al.: JGW implies that intrinsic stock productivity may change over time and that it may relate to small spawner sizes in successive years, but does not provide evidence that this has occurred. First, there is no temporal baseline for comparison for the stock that he uses in his example (upper Salmon River); i.e, egg-smolt survival for this stock was not estimated during the 1960s. Also, the statement ignores the evidence from Chapter 9, that changes in Snake River aggregate smolt/spawner productivity and survival rate and between pre-1975 and post 1975 (if any) were small relative to overall declines in adult-to-adult productivity and survival rate.

JGW cites Kiefer et al. (1997) estimates of egg-to-smolt survival rate from the upper Salmon River from 1988-1994 which averaged 2.2%, and states that if levels were this low in the 1960s, the populations would have crashed. However, survival rate estimates reported in the literature are quite sensitive to the life stages indexed, and the methods and assumptions used; extrapolations should be made with caution.

At a typical fecundity (for this stock) of 5,000 eggs per female, 2.2% egg-to-smolt survival rate delivers 110 smolts to the upper end of the hydrosystem, and 2% SAR to the spawning grounds delivers about 2 adults (1 female, i.e., replacement). In their 1994 annual report, Kiefer and Lockhart (1997; p. 38) indicate that average egg-to-smolt survival rate for the headwaters upper Salmon River for brood years 1987 to 1992 was 4.1% or 3.6%, depending on which method was used. At these rates, 180 to 205 smolts would be delivered to the upper end of the hydrosystem, and about 1% SAR to the spawning grounds would result in replacement. SARs of 1%-2% from the upper end of the hydrosystem to the spawning grounds are conceivable for the 1960s based on Raymond (1988) SAR estimates of 3%-6%, which were indexed lower in the system (and accounted for river harvest). Without a careful accounting of method and life stage differences, the recent upper Salmon River egg-to-smolt survival rate estimates seemingly do not provide evidence that the rates have decreased for this stock, nor do they indicate depensation has not occurred.

Source:
Kiefer, R.B. and J.N. Lockhart. 1994. Intensive evaluation and monitoring of chinook salmon and steelhead trout production, Crooked River and upper Salmon River sites. Annual Report 1994. BPA Project 91-073.

End of comment on Section A.3.2 (Stock Productivity) by Williams and response by Schaller et al.

A.3.3. Hypotheses for Delayed Mortality

Extra mortality is any mortality occurring outside of the juvenile migration corridor that is not accounted for by either: 1) spawner-recruit relationships; 2) estimates of direct mortality within the migration corridor; or 3) for the Delta model only, common year effects affecting both Snake River and Lower Columbia River stocks.

A.3.3.1 Extra Mortality is Hydro-Related

Hypothesis:

The completion of the Federal Columbia River Power System in the late 1960s through the mid-1970s and subsequent operation, has increased the direct and delayed mortality of juvenile migrants, which resulted in considerably sharper declines in survival rates of Snake River spring and summer chinook stocks (over the same time period), than of similar stocks which migrate past fewer dams and are not transported.

Rationale:

Several possible mechanisms were identified from the literature in Weber et al. (1997) that may explain delayed mortality of smolts that are transported and those that migrate in-river through the hydropower system.   These include: altered saltwater entry timing which is poorly synchronized with the physiological state of the smolts (CBFWA 1991; Fagurlund et al. 1995); stress from crowding and injury (including descaling--Basham and Garrett 1996; Williams and Matthews 1995) during bypass, collection, holding and transport (Mundy et al. 1994); increased vulnerability to disease outbreak (e.g., BKD and fungal infection) due to stress and injury (Mundy et al. 1994; Raymond 1988; Williams 1989); and increased vulnerability to other stressors in the environment or to predation, particularly by northern squawfish (Mundy et al. 1994).

Evidence for delayed mortality due to hydrosystem passage comes in part from the PATH 1996 conclusions on the retrospective analysis (Marmorek and Peters 1996), stock-recruitment comparisons (Schaller et al. 1996) and the MLE retrospective model (Deriso et al. 1996). MLE estimates of Φ, which include direct and delayed passage mortality components, were correlated with water travel times experienced during the smolt outmigration; total mortality of Snake River spring/summer chinook tended to be highest in low flow, low spill years which had higher proportions of smolts transported. Passage models which assumed low delayed mortality of transported smolts (CRiSP T2) had the poorest fit in the MLE (e.g., Fig. 5-5 of Deriso et al. 1996).

The transportation of juvenile salmon through the mainstem Columbia and Snake River hydropower system began as an experimental program in the late 1960s and has been the principal method for improving mainstem survival since the early 1980s (Mundy et al. 1994). Its justification rests on experimental results indicating transported fish return as adults in greater proportions than their non-transported counterparts. Although there are problems with transportation research (Mundy et al. 1994), it appears that transported Snake River spring/summer chinook survive at higher rates than their in-river counterparts in most years studied.

Transportation has been in operation ever since Lower Granite Dam was completed in 1975 and has been fully operational since 1977, but Snake River spring/summer chinook have continued to decline. These declines have shifted focus from comparisons between the relative survival of transported and in-river fish to questions about the absolute survival of transported fish. Recently, two documents (Mundy et al. and Toole et al. 1996) have stressed the importance of evaluating the absolute survival of transported juvenile chinook salmon. Furthermore, the PATH hydro group recommended an interim goal of 2% to 6% smolt to adult return rates (SAR) (Toole et al. 1996).

The estimated SAR rates of transported Snake River wild smolts have been considerably less than the SARs prior to FCRPS completion, and less than the recent SARs of a similar downriver stock, Warm Springs River (Raymond 1988; Weber 1996; Weber et al. 1997). The SARs of transported Snake River wild spring/summer chinook estimated from coded wire tags (1975-1990) and PIT tags (1989-1995) indicate that transported fish rarely, if ever, meet the goal of 2% to 6% SAR (Figure A.3.3.1-1).
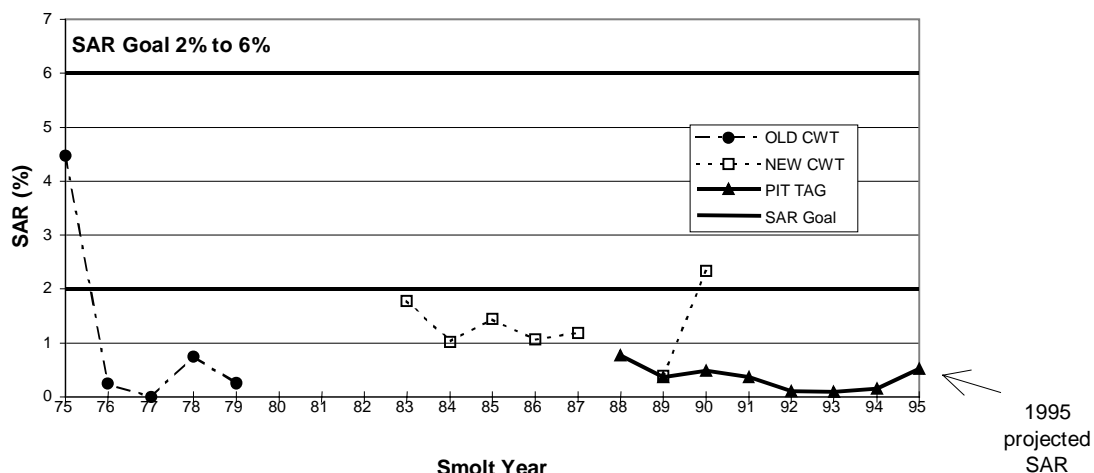
Figure A.3.3.1-1: SAR of Transported Wild Spring/Summer Chinook

Evidence for delayed mortality of transported smolts relative to those that pass in-river through the hydrosystem also comes from a comparison of in-river survival estimates and the inverse of the T/C ratio. If post-hydrosystem mortality were roughly equal between the transported and in-river groups as hypothesized by Williams et al. (1997), the points should scatter around the 1:1 line. However, the scatter of points from the 1968-1979 transportation and in-river survival studies tended to fall to the right and below the 1:1 line (Figure A.3.3.1-2), which supports the hypothesis that delayed mortality was greater for transported fish than for the controls (i.e., that D<1).

Preliminary analysis of return rates of PIT tagged wild smolts from 1994 to 1995 further suggests that delayed mortality of in-river migrants may be related to route of passage through the hydropower system (Weber et al. 1997). Smolts that were detected (i.e., were bypassed) two or more times returned at lower rates than those detected one or zero times (Figure A.3.3.1-3). These wild smolts represented fish that were released above Lower Granite Dam and were not collected for tagging in the fish bypass system at Lower Granite Dam. Based on PATH estimates of direct mortality through bypass, spill and turbine routes, these results suggest that delayed mortality increases as a function of the number of times a fish is bypassed. In addition, the T/Cs estimated using these in-river fish was 1.80 in 1994 and 1.04 in 1995 (partial returns).

Survival estimate, adjusted for appropriate number of dams, vs. Transport based survival estimate. 1968-79, excluding 1974 and 1977
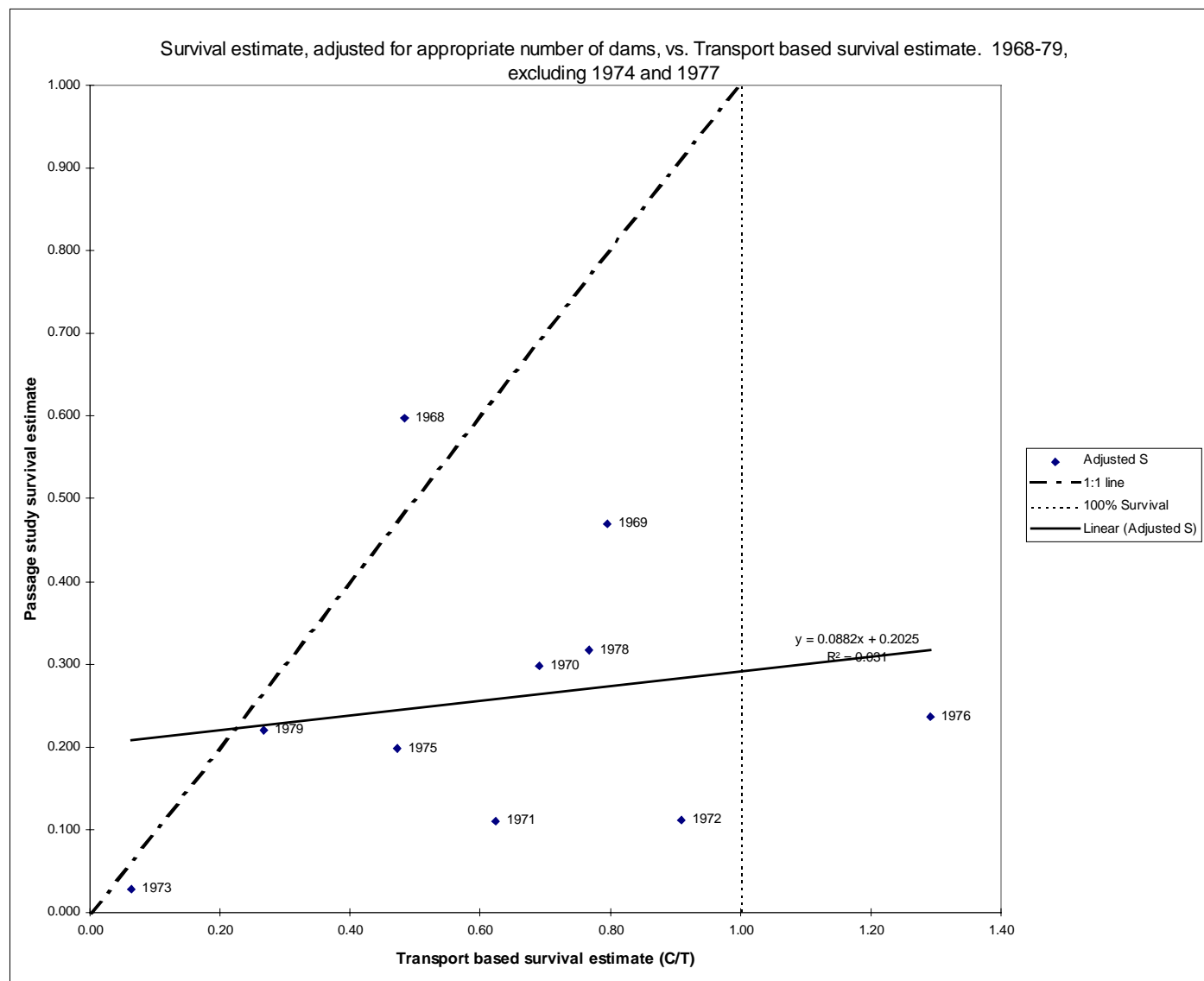
Figure A.3.3.1-2:   Survival estimate, adjusted for appropriate number of dams, vs. Transport based survival estimate (1968-1979, excluding 1974 and 1977)
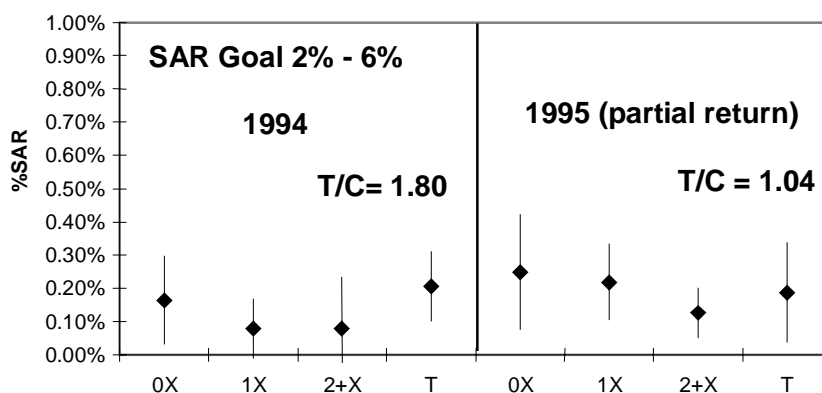
Figure A.3.3.1-3:   SAR by detection history, wild yearling chinook.

The use of a common year effect parameter in the MLE analysis (Deriso et al 1996) reflects evidence that the estuary and early ocean conditions do not have a systematically different effect on survival for stream-type chinook stocks across regions of the interior Columbia River Basin. This is reasonable in view of similarity of these stocks, the overlap in time and space of these stocks during their early ocean residence (and beyond), and the broad-scale nature of climatic influences described in the literature.

There are several lines of evidence suggesting that the interior Columbia Basin stocks are exposed to similar estuary and ocean conditions, particularly during the critical first year. Beamish and Bouillon (1993) and others provided evidence that indices of climate over the north Pacific Ocean may play an important role in production of different species of salmon originating over a wide geographic range. In a review paper, Anderson (1996) concluded that a warm/dry regime favors stronger year class strengths of many Alaska fish stocks while cool/wet regime favors stocks on the West Coast of the lower United States. Deriso et al. (1996) found evidence of a common year effect for all index stocks of stream-type chinook from the Snake River and lower Columbia River regions. Of the lower Columbia River stocks in this analysis, at least the John Day River and Warm Springs River spring chinook smolt timing appears very similar to that of Snake River spring and summer chinook. Smolts of these lower Columbia River, Snake River and upper Columbia River stocks migrate through the mainstem to the estuary primarily in late April and May (Lindsay et al. 1986, 1989; Raymond 1979; Hymer et al. 1992; Mains and Smith 1964). Current hypotheses regarding ocean survival of Pacific salmon generally focus on the juveniles' critical first months at sea (Pearcy 1988, 1992; Lichatowich 1993), where juveniles of these index stocks are most likely to overlap in time and space. Year class strength for these spring and summer chinook is apparently established, for the most part, within the first year in the ocean, as evidenced by the ability of fishery managers to predict subsequent adult escapements from jack counts (e.g., Fryer and Schwartzberg 1993).

Although ocean recoveries of coded wire tagged spring/summer chinook are infrequent (Berkson 1991), the few recaptures (62 recoveries from 8 release years) from both Snake River (21 recoveries) and lower Columbia River (41 recoveries) hatchery stocks were widely scattered from California to Alaska ocean fisheries (PSMFC unpublished data). The average annual proportion of CWT recoveries from ocean fisheries north and south of the Columbia River mouth appears to be similar between the Snake and lower Columbia hatcheries (Figure A.3.3.1-4). Since it appears that Columbia Basin stream-type chinook share a common estuary and nearshore ocean environment and a more common ocean distribution than stocks evaluated by Beamish and Bouillon (1993), it seems very unlikely that differential estuary and ocean conditions themselves (i.e., apart from differences in delayed effects due to juvenile migration) would have had a systematically different effect on survival.
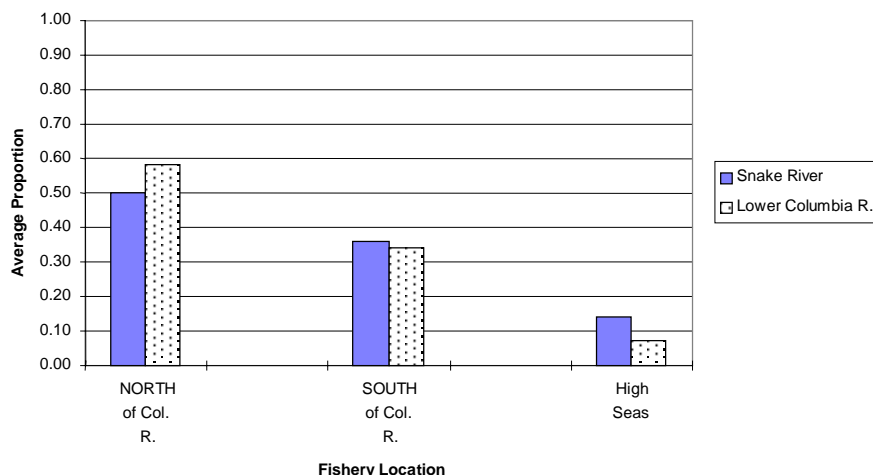
Figure A.3.3.1-4: Observed CWT Ocean Recoveries of Snake River and Lower Columbia River Hatchery Spring Chinook. Source: Weber et al. (1997

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Representation in the Delta Model

Under the hypothesis that λ_n, post-Bonneville survival factor of non-transported smolts, is related to V_n, in-river survival of non-transported smolts, the linkage of passage model output to the BSM is done as follows.

The passage + extra mortality rate m can be converted into a survival fraction, which can be represented as the product of system survival (ω) and delayed survival factor of non-transported fish (λ_n) (as in Equation a5):

$$\exp(-m) = \omega\lambda_n \qquad\qquad\qquad \text{[Eq. A.3.3.1-1]}$$

With an estimate of m and an estimate of system survival from a passage model, λ_n can be estimated by rearranging the above equation:

$$\lambda_n = \exp\left[-m - \ln(\omega)\right] \qquad\qquad\qquad \text{[Eq. A.3.3.1-2]}$$

The value of λ_n for a particular retrospective year ($\lambda_{n,r}$) can be estimated from the retrospective BSM model m for that year (m_r) and the passage model system survival estimate for that retrospective year (ω_r). The retrospective annual delayed mortality factor of non-transported fish (M_r^*) is defined as 1 - $\lambda_{n,r}$. To represent this hypothesis about delayed mortality in the BSM, we assume the delayed mortality factor [1-λ_n] for non-transported smolts is proportional to in-river mortality [1- V_n] of non-transported smolts. The prospective annual delayed mortality factor of non-transported fish (M_y^*) is calculated according to

$$M_y^* = M_r^* \cdot \frac{1 - V_{n,y}}{1 - V_{n,r}}$$ [Eq. A.3.3.1-3]

where $V_{n,y}$ and $V_{n,r}$ are prospective and retrospective direct in-river survival, respectively, from a passage model. The estimated prospective delayed survival factor of non-transported fish $(\lambda_{n,y})$ is then

$$\lambda_{n,y} = 1 - M_y^*$$ [Eq. A.3.3.1-4]

The retrospective and prospective λ_n's, along with the retrospective and prospective system survivals are then used in Equation A.3.2-10 above to derive the prospective passage plus extra mortality rate (m_y) to use in prospective BSM runs.

The delta model in prospective mode can be written:

$$\ln(R_y) = (1 + p)\ln(S_y) + a - bS_y - m_r + \ln(\omega_y / \omega_r) + \ln(\lambda_{n,y} / \lambda_{n,r}) + \delta_y + \varepsilon_y$$ [Eq. A.3.3.1-5]

The retrospective water year coupled to each prospective year is chosen from brood years 1975-1990. The prospective system survival term ω_y is specified in the input file.

Comment on Representation of Delayed Mortality in the Delta Model by Anderson and response by Wilson et al:

In words the Delta model in-river fish delayed mortality hypothesis is:
> "the post-Bonneville survival factor of non-transported smolts is related to the in-river survival of non-transported smolts"

and the mathematical translation of the hypothesis is given as

[20]

The designation y and r are for prospective and retrospective years, presumably with similar flow regimes. Although the relationship between the FLUSH verbal and mathematical hypotheses may be intuitive, there are uncertainties with the approach. First, there is no clear identification of how retrospective and prospective in-river and estuary/ocean survivals are actually linked in terms of mechanisms. Second, the mathematical form, Eq [20], is not unique since the verbal hypothesis could equally be expressed in terms of survivals leading to the alternative expression

[21]

A problem emerges because Eq [20] and Eq [21], two equally viable models, have different characteristics. While in both cases the sensitivity of extra mortality to in-river survival depends on the retrospective conditions, it does so in exactly opposite ways in the two formulations. For the mortality-based hypothesis mortality, Eq [20], the incremental change in post-Bonneville survival is

$$[22]$$

so for a unit increase of prospective in-river survival, $V_{n,r}$, smaller values of the retrospective post-Bonneville survival, $\lambda_{n,r}$, equate to a smaller increases for prospective post-Bonneville survival $\lambda_{n,y}$.

Response by Wilson et al. In the description under equation (22), $V_{n,r}$ is misidentified as prospective in-river survival; it is actually retrospective in-river survival. The main point is that $V_{n,r}$ does not increase or decrease with management actions; it is an estimate of past survival for a specific year, and depends only on assumptions about past passage conditions (e.g. TURB1, TURB4, or TURB5).

Comment by Anderson continues:

For the survival-based hypothesis, expressed by Eq [21], the incremental change in post-Bonneville survival is expressed by the equation

$$[23]$$

and this has exactly the opposite behavior of Eq [22]. That is, smaller values of the retrospective post-Bonneville survival, equate to larger prospective survival increases.

The salient point here is that the FLUSH hydro related mortality hypothesis expressed by Eq [20] is a non-unique case with unique consequences as to how information from past conditions alters future predictions. Other equally plausible and simpler expressions of the hypothesis give opposite responses. Thus, the hypothesis as it stands is incomplete in that it is not based on first principles and its consequences depend on the specific but non-mechanistic functional form. If stress were the factor relating the two survivals as expressed by Eq [9] then the relationship based on survivals through Eq [21] would be the more mechanistically correct.

Since the hydro-related mortality factor includes ω which depends on D, and D is problematic because it forces a condition on the transport survival λ_T that is apparently unrealistic, the hypothesis needs to be reconsidered.

End of comment on Representation of Delayed Mortality in the Delta Model by Anderson and response by Wilson et al.

Representation in the Alpha Model

One feature of the alpha model that is different from the delta model is that Hinrichsen and Anderson include in their definition of "extra" mortality (α) climatic variables (E,F) not related to hydro, whereas in the delta model, common year effects are not included in their definition of post-Bonneville survival factors.

Representation of the hydro-related extra mortality hypothesis in the Alpha model is analogous to the approach used in the Delta model. In the Delta model it is assumed that retrospectively 1- λ_n , post-Bonneville mortality factor of non-transported smolts, is proportional to 1- Vn, in-river mortality of non-transported smolts. In the Alpha model, the *STEP* term is assumed to be proportional to 1-Vn:

$$1 - \exp[-STEP_r] \propto 1 - \overline{Vn}_r \qquad \text{[Eq. A.3.3.1-6]}$$

where the average is taken over brood years 1975-1990 and during any prospective year *y* the same proportionality coefficient applies to

$$1 - \exp[-STEP_y] \propto 1 - Vn_y \qquad \text{[Eq. A.3.3.1-7]}$$

which we implied that

$$STEP_y = -\ln[1 - (1 - \exp(-STEP_r))(1 - Vn_y) / (1 - \overline{Vn}_r)] \qquad \text{[Eq. A.3.3.1-8]}$$

The alpha model in prospective mode can be written as

$$\text{[Eq. A.3.3.1-9]}$$

where the α_y is given by Equation A.3.2-15 , except we use Equation A.3.3.1-8 to calculate a prospective value for *STEP*. The prospective Vn, *D,* and *P* are specified in the input file. In prospective years, there are new *F,E* variables according to the particular climate hypothesis.

Comment on Representation of Delayed Mortality in the Alpha Model by Anderson:

> This hypothesis, like the Delta equivalent, has no biological mechanism relating in-river and post-Bonneville survivals.

End of comment on Representation of Delayed Mortality in the Alpha Model by Anderson.

A.3.3.2 *"BKD" Hypothesis for Extra Mortality*

Hypothesis:

Extra mortality is not related to the hydropower system or climate conditions and is here to stay.

Rationale:

Although the hydropower system causes some direct mortalities that can account for some of the decreased returns since its construction, the direct mortality that results from the hydropower system under the present configuration with bypass systems at most dams is far from sufficient to account for the present low adult return rates. Alternative hypotheses to explain the continued low returns have generally fallen into two categories: 1) the hydropower system is responsible for the low returns because fish, if not dying directly due to the hydropower system, die indirectly after having migrated through or been transported around the system; and 2) some cyclical change in climate that occurred about 1976/77 has created unfavorable rearing and/or ocean conditions which has led to a much lower adult return rate than in the 1950s and 1960s. Both of these general categories of hypotheses presume that the adult returns will increase with changes in the underlying factors that control the stocks. In the case of hypotheses related to hydropower, removal of dams will cause a substantial increase in adult returns. In the case of cyclical climate hypotheses, once the climate changes back to a favorable conditions, adult returns will substantially improve.

The change in stock viability hypothesis (there are likely a number of scenarios by which this could occur) presumes that, at least for the next several decades, stock returns will not substantially increase. Stock viability likely changed as a direct or indirect result of construction of the hydropower system in the 1970s. However, once the viability decreased, no or only slight increases in viability would occur with a favorable change in climate or the hydropower system. This hypotheses provides a pessimistic outlook for the stocks. The following are a couple of scenarios under this hypothesis that suggest how this might occur:

1) Stock sizes have gone so low in some streams that they are not likely to rebound. This could result from a low number of juveniles produced in any one year, but with predator numbers remaining equal to past levels, thus, the predation rate on juveniles is higher. Further, it is possible that adult returns from any one run year are not high enough to return needed nutrients and fertilizer to support the parr in the streams from the previous return years. In a recent report to BPA, Kiefer and Lockhart (1997) estimated the egg to smolt survival of juveniles from the upper Salmon River to the head of Lower Granite Reservoir ranged from 0.5 to 5% from 1988 through 1994 and averaged 2.6%. Based on levels of adult catch and estimated in-river survivals through the hydropower system during the 1960s, egg to smolt survivals would have been much higher than these historically or the stocks would have crashed long ago. This suggests the viability of eggs to smolts is much lower than historically.

2) Disease (most notably bacterial kidney disease [BKD]) was transmitted to wild stocks with the implementation of hatchery programs that began in the Snake River Basin after the initial construction of the Snake River dams. The disease was either new to the stocks or a different and more virulent form that resulted from hatchery practices and it was transmitted to the wild fish from the hatchery fish. Wild fish in the basin now have the new form that is readily transmitted and likely results in a high death rate for fish that have it. The disease affects all fish from the basin and doesn't act on the individuals until they reach the estuary or early ocean. USGS researchers have found BKD at some level in all Snake River basin wild spring/summer chinook salmon stocks. Additionally. USGS researchers conducted studies in 1997 (unpublished) to determine mortalities in groups of fish subjected to either no stresses (control), 1,

2, 4, or 8 cumulative stress incidences that would represent the cumulative passage through 1, 2, 4, or 8 dams. All fish had relatively low survivals. Control fish survival was approximately 50%; whereas the survival of all treatment groups was 10% or less. From these initial studies, they concluded that the type of river conditions that Snake River spring/summer chinook salmon encounter is not likely to affect survivals substantially. If BKD is the causative agent of death, then the fish will likely die with or without the majority of hydropower dams in place.

Comment on Section A.3.3.2 (BKD hypothesis) by Schaller et al.:

> The BKD hypothesis and rationale are incomplete to the extent that they cannot be evaluated or assigned any weight of evidence. There is no literature cited. Proposers of this hypothesis still need to: 1) identify those possible mechanisms that would make Snake River spring/summer chinook stocks *more susceptible* to BKD than lower river stocks, *unrelated to* the stress and crowding which occurs during collection, holding, bypass and transportation; 2) provide a biological rationale for those mechanisms, including literature citations; and 3) provide evidence in support of that rationale.
>
> Until this is made more specific, it seems counterproductive for PATH to continue evaluating this hypothesis.

End of comment on Section A.3.3.2 (BKD hypothesis) by Schaller et al.

Representation in the Delta Model

For the situation where extra mortality is here to stay, $\lambda_{n,y}/\lambda_{n,r} = 1$. The delta model in prospective mode can be written as

$$\ln(R_y) = (1+p)\ln(S_y) + a - bS_y - m_r + \ln[\omega_y / \omega_r] + \delta_y + \varepsilon_y \qquad \text{[Eq. A.3.3.2-1]}$$

The system survival in prospective years, ω_y, will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions. The retrospective water year coupled to each prospective year is chosen from brood years 1975-1990.

Comment on Representation of BKD hypothesis in Delta Model by J. Anderson:

> The hypothesis that the recent increase in extra mortality is related to the evolution of disease in the Snake River stocks and is here to stay is expressed by the condition $\lambda_{n,y}/\lambda_{n,r} = 1$. Time precludes a complete analysis of this hypothesis but since it contains ω which depends on D and D is problematic because it forces a condition on the transport survival λ_T, the hypothesis is unpredictable and it is possible that it may generate unrealistic results because of its structure.

End of comment on Representation of BKD hypothesis in Delta Model by J. Anderson.

Representation in the Alpha Model

The hypothesis that extra mortality is here to stay is represented in the Alpha model by assuming that the *STEP* component of "extra" mortality is here to stay. In this case the alpha model in prospective mode can be written as in eq. A.3.3.1-9, except that the α_y is calculated with assumption that $STEP_y=STEP_r$. In prospective years, there are new *F,E* variables according to the particular climate hypothesis. The alpha value in prospective years, α_y , will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions.

Comment on Representation of BKD hypothesis in Alpha Model by Anderson:

> The hypothesis gives a first order relationship of disease on extra mortality. It could be improved by a more detailed formulation of disease dynamics and the vectors of transmission of disease between hatchery and wild fish and between transported and non-transported fish.

End of comment on Representation of BKD hypothesis in Alpha Model by Anderson.

A.3.3.3 Regime Shift Hypothesis for Extra Mortality

Hypothesis:

Extra mortality is not related to hydro, but due instead to an interaction with a cyclical climate regime shift with a period of 60 years, crossing 0 in brood year 1975.

Rationale:

Widespread ecological changes related to interdecadal climate variations in the Pacific have been observed in this century. Dramatic shifts in many marine and terrestrial ecological variables in western North America coincided with changes in the physical environment in the late 1970s (about 1977). The 1977 regime shift is not unique in the climate record nor in the record of North Pacific salmon production. Signatures of an interdecadal climate variability are detectable in many Pacific basin ecological systems (Mantua et al. 1997). Among the salmon species shown to have interdecadal variability were Alaskan sockeye, Alaskan pink salmon, Columbia River spring chinook, and Washington-Oregon-California (WOC) coho. While the climate regime from 1977-present has favored Alaskan sockeye and pink salmon production, it has been associated with decreased production of Columbia River spring chinook and WOC coho (Hare et al 1997).

Comment by Schaller et al.:

> The "regime shift" hypothesis is really a *selective* regime shift hypothesis. As framed, it does not address why the climate regime shift might be *systematically* reducing survival rates more for Snake River stocks than for lower river stocks, *unrelated* to hydrosystem passage.

> The point being made here is that climate may affect productivity of salmon stocks *in general*. What would be needed as evidence to support the selective regime shift hypothesis would be evidence that Snake River spring/summer chinook stocks are particularly vulnerable to climate variability, unrelated to their unique hydrosystem experience.

Note also that the stock performance measures being referenced in this rationale are of abundance (catch or run-size). Productivity and survival rate data would provide a stronger basis in support of a hypothesis, since abundance is a function of escapement and exploitation policies, as well as productivity and survival. There is recent evidence that changes in abundance indicators for several Alaskan salmon stocks may be explained by changes in escapement and harvest management policies (Farley and Murphy 1997), rather than climate patterns.

End of comment by Schaller et al.

The PDO is an index of climate based on the North Pacific sea surface temperature pattern since 1900. It is an index that other physical and biological tend to follow on the interdecadal time scale. The signature of PDO appears in the Gulf of Alaska Air Temperature, British Columbia coastal sea surface temperature, Scripps Pier sea surface temperature, Gulf of Alaska stream flow, and British Columbia/Washington stream flow. It also appears in other measures of climate such as the North Pacific sea level pressure.

A regime shift occurs every 25-30 years, whenever the Pacific Decadal oscillation (PDO) makes a polarity switch (positive to negative or visa versa). We simulate the PDO using a square wave with a period of 60 years. In this century, the polarity switches occurred in 1925 (to warm/dry climate), 1947 (to cold/wet climate), and 1977 (to warm/dry climate). These years correspond to Minobe's (1997) analysis of reconstructed continental surface temperatures, which showed interdecadal oscillations (of period 50-70 years) over the last 3 centuries. The next polarity switch is modeled to occur in 2007 (spring chinook brood year 2005).

The biological effects of these regime shifts have been dramatic, changes in catch of Alaskan pink and sockeye salmon decreased by about 57% in 1947, and increased by about 230% during the 1977 climate regime shift (See Table below). Generally speaking Alaskan salmon and Pacific Northwest salmon have run sizes that fluctuate in reverse to one another (Hare et al. 1997). The 1947 shift, which was bad for Alaskan stocks, was good for Columbia River upriver spring chinook, which showed a 49.5% increase. The 1977 regime shift, though good for Alaskan stocks, was bad for Columbia River upriver spring chinook, which showed a 53.1% decrease (See Figure below).

Table A.3.3.3-1. Percent change in mean catches of four Alaskan stocks and run sizes of Columbia River upriver spring chinook following major PDO polarity changes in 1947,1977. Mean catch levels (run sizes for Columbia stock) were estimated from intervention models fitted to the data, using a 1-year lag for both pink salmon stocks, and 2-year lag for western sockeye, and a 3-year lag for central sockeye, and a 2-year lag for spring chinook. (Alaskan stocks from Mantua et al. [1997]).

Salmon stock	1947 step	1977 step
Western Alaskan sockeye	-32.2%	+242.2%
Central Alaskan sockeye	-33.3%	+220.4%
Central Alaskan pink	-38.3%	+251.9%
Southeast Alaskan pink	-64.4%	+208.7%
C.R. upriver spring chinook (run size)	+49.1%	-55.5%

Comment by Schaller et al.:

This summary table appears to be evidence that *different species* (sockeye and pink salmon) from

broad geographic areas (most of Alaska) responded similarly (using mean catch statistics) to climate regime shifts (major PDO polarity). It could be used to support the common year affect of the delta model. Possible mechanisms, rationale and evidence are needed to explain why, under this hypothesis, that very similar, adjacent stock groupings of stream-type chinook would have vastly different responses to climate regimes, unrelated to their unique hydrosystem experience.

End of comment by Schaller et al.:

Figure A.3.3.3-1: Regime shifts are apparent in the run size data of upriver spring chinook of the Columbia Basin (data from ODFW and WDFW 1995). The average levels (dashed lines) were calculated for the various climatic regimes (pre-1947, 1947-1977, and post-1977). Based on the fitted intervention model, the 1947 regime shift resulted in a run that increased by 49.1%, and the 1977 regime shift resulted in a run that decreased by 55.5%. To approximate outmigration year, the year of adult return was lagged by two.

Comment by Schaller et al.:

> This is a very simplistic example. The data used in Figure A.3.3.3-1 are for the aggregate wild and hatchery runsize of upriver spring chinook. No attempt was made here to sort out effects of changing mainstem harvest rates (range 0.03 to 0.86), or of proportion wild fish in the run. If this figure is evidence of the influence of a regime shift on upriver aggregate spring chinook (including both Snake River and lower river index stocks), it does not provide evidence in support of a selective regime shift hypothesis.

End of comment by Schaller et al.:

We used a simple intervention analysis of the upriver spring chinook runsize (1936-1992 outmigration years) to determine whether the regime shifts and anthropogenic effects were important. There are two warm/dry periods with this record: 1936-1947 (REGIME 1) and 1977-1992 (REGIME3), and one cold/wet period 1948-1976 (REGIME2). To estimate the post-1947 anthropogenic effect on 1977-present run size, we compared the average run size during REGIME1 and REGIME3, assuming that the run sizes would be similar during these time periods in the absence of anthropogenic influence. We bounded the effect of the climate regime shift on the run size by estimating a model under two different assumptions: (1) there was no anthropogenic effect during REGIME 2, and (2) the anthropogenic effect during REGIME 2 equaled their effect during REGIME 3. Under (1), we obtain a lower bound for the effect of the climate regime shift, *LCLIM*, the difference in average run sizes during REGIME 2 and REGIME 1. Under (2) we obtain an upper bound on the effect of the regime shift, *UCLIM*, the difference in mean run sizes between REGIME 2 and REGIME 3.

The model used to compute the anthropogenic and climate effects was:

$$RUN_t = REGIME_t + N_t$$
$$N_{t+1} = \phi N_t + \varepsilon_t$$

Where
RUN_t = The Run size corresponding to outmigration year t
$REGIME_t$ = Factor variable containing three runsize levels (*REG1, REG2, REG3*) during the three climate regimes
N_t = Noise following AR(1) process
ϕ = AR(1) coefficient for noise process
ε_t = $N(0, \sigma^2)$ gaussian noise process

The post-1947 anthropogenic effect on the 1977-present run size, *ANTHRO*, was computed as *REG1-REG3*, the lower bound for the climate effect, *LCLIM*, is calculated as *REG2-REG1*, and the upper bound, *UCLIM*, was computed as *REG2-REG3*.

Table. A.3.3.3-2. Parameter estimates of intervention model. The ratio of the climate to hydrosystem effects on spring chinook runsize ranges from 3/2 to 5/2. Number of observations = 56, number of parameters = 4, d.f. = 52.

parameter	Value	Std. Error	t-value	p-value
LCLIM, lower bound on effect of climate shift.	57.85	23.43	2.47	0.0168

UCLIM, upper bound on effect of climate shift.	97.47	19.47	5.01	6.71e-6
ANTHRO, post-1947 anthropogenic effect on 1977-present run size.	39.63	26.19	1.51	0.1363
ϕ, AR(1) parameter.	0.388	0.13	2.96	0.0046

The climate effect, estimated between 57.85 and 97.47, was significant at the 0.05-level and it outweighed the anthropogenic effect (39.63) by a ratio of 3/2 to 5/2. The anthropogenic effect, giving a t-value of 1.51, was not significant at the 0.05-level (p-value = 0.1363).

Hare et al. (1997) performed a principle components analysis (PCA) on salmon stocks from Alaska to California which demonstrated the inverse relationship between the catch of Gulf of Alaska stocks and that of West Coast (south of Vancouver Island) stocks. The first principal component explained 69 percent of the variance in the salmon catch. The inverse principle component loadings (expressed as correlations between the salmon catch and principle component score) together with the principal component scores showed interdecadal salmon production regimes within the Pacific basin with an inverse relationship between Northern (Alaska) and southern (West Coast) stocks.

Pearcy (1992) found a relationship between the 1977 regime shift and production of adult hatchery coho. Prior to 1976, there was a positive relationship between smolts released and adult production. From 1976-1986, the relationship became negative. This loss of coho production was accompanied by increased sea level and sea temperatures in the California Current System and reduced southward transport and weak upwelling along the coast (Pearcy 1992).

The marine ecological response to PDO may start with the plankton at the base of the food chain and work its way up to top-level predators such as salmon (Francis et al. 1997). After the 1977 regime shift, there was a zooplankton biomass increase and a re-distribution around the Subarctic gyre, creating favorable feeding conditions for migrant salmon smolts (Brodeur and Ware 1992, Sugimotoa and Tadokoro 1997). Conversely, off the West Coast , there was a dramatic decrease in zooplankton production due to stratification of the California Current waters and loss of advective products from the westwind drift (Roemmich and McGowan, 1995). This relatively barren ocean environment was unfavorable for West Coast smolts (Hare et al. 1997).

Some phytoplankton and zooplankton population models are sensitive to upper-ocean and mixed-layer depths and temperatures, and these are linked with the fluctuations in PDO. One such model has successfully simulated the increase in Gulf of Alaska productivity as a response to 20-30% shoaling and 0.5-1 degrees Celsius warming of the mixed layer (Polovina et al. 1995). Increased PDO generally brings enhanced stream flows and nearshore ocean mixed layer conditions that a favorable to productivity, but generally decreased productivity for Pacific Northwest salmon (Mantua et al. 1997).

There is no agreement on the origin of the decadal climate variability (Ware 1995). Schlesinger and Ramankutty (1994) speculated that the signal might be caused by a low frequency oscillation in the North Atlantic thermohaline circulation. Friis-Christensen and Lassen (1991) suggest that long term global temperatures by respond to lower frequency variation in the solar cycle length, which may indicate change in total solar energy output.

Connection to higher frequency climate fluctuations

The PAPA drift (explained below in A.3.4) appears to make a polarity switch whenever there is a polarity switch in the PDO.

PAPA Drift Crossings (Actual Years)

TO WARM/DRY	1918*	1937	1958	1974*
TO COLD/WET	1929		1946*	1963

The *s correspond to regimes shifts which were estimated by others to occur at 1925, 1947, and 1977 (actual years) (Mantua et al. 1997). The cycle length of the PAPA drift is approximately 18 years – about 1/3 the period of the PDO.

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Comment on Section A.3.3.3 (Regime Shift hypothesis) by L. Botsford:

> It is clear that there was a change in the atmosphere in the north east Pacific in the mid 1970s, and considerable evidence that that change had a positive influence on biological productivity in the Gulf of Alaska. There are also mechanistic reasons to believe that there is an inverse relationship between the flows in the Alaska Coastal Current and the California Current. However, whether the change in the 1970s had an effect on salmon stocks in Washington, Oregon and California is just a hypothesis, and evidence regarding that hypothesis must come the area where these fish are found. The Columbia River stocks spend their early life in the California Current, south of the bifurcation of the West Wind Drift, so if that period is important we need evidence from salmon stocks in the California Current.

> One piece of evidence an effect of the regime shift on CCS stocks is the decline of Oregon hatchery survivals in the 1970s described Pearcy (1997). However, that same decline is not seen in the Coronado-Hernandez thesis (Fig. 11). Another piece of evidence (though marginal because of the location) is the decline in survivals of chinook stocks in the Georgia Strait (Beamish 199x). There is also a marginal correlation between zooplankton densities off southern California (where there are no salmon), and zooplankton densities in the Gulf of Alaska (Brodeur, et al. 1996?).

> Some of the relationships presented in the current document are not rigorous evidence. In the paper by Mantua, et al. (1997) no "correlations" are computed between the PDO and Columbia River stocks nor Washington-Oregon-California coho. There are also no correlations between a variable representing the regime shift and California Current salmon in Francis and Sibley (1991) nor in the Anderson (1996) review. As far as I know, none of the referenced papers contain statistical evidence relating California Current salmon stocks to the regime shift in the Gulf of Alaska.

End of comment on Section A.3.3.3 (Regime Shift hypothesis) by L. Botsford.

Representation in the Delta Model

For this hypothesis, the delta model is written exactly as in eq. A.3.3.2-1, except that the retrospective water year chosen for a given prospective year is one which occurred during the same phase of the cycle. For example, until brood year 2005 the coupled retrospective years are chosen from brood years 1975-1990, then from brood year 2006 for the next 30 years the coupled retrospective years are those chosen from brood years 1952-1974 (1952 is first year of S/R data). The system survival in prospective years, ω_y, will be calculated based on input M and P values, but the D values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current D conditions.

Comment on representation of Regime Shift hypothesis in Delta Model by J. Anderson:

> The hypothesis has the same problems as other FLUSH/Delta hypotheses in that delayed transport mortality is driven by in-river survival according to Eq [19]. Consequently it is a variant of the hydro related extra mortality model and the hypothesis needs to be further considered.

End of comment on representation of Regime Shift hypothesis in Delta Model by J. Anderson.

Representation in the Alpha Model

The alpha model is written exactly as in eq. A.3.3.1-9, except that the *STEP* value chosen for a given prospective year is one which occurred during the same phase of the cycle. For example, until brood year 2005 the *STEP* is the one applicable to brood years 1975-1990, then from 2006 for the next 30 years the *STEP=0*, which is the one applicable to brood years 1952-1974 (1952 is first year of S/R data). In prospective years, there are new *F,E* variables according to the particular climate hypothesis. The alpha value in prospective years, α_y , will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions.

General comments on Section A.3.3. (Extra Mortality Hypotheses) by Anderson and Response by Wilson et al:

> The model systems are both complex and although they are generally based on the same data sets they are derived from different philosophies. The FLUSH/Delta model system was derived without explicit hypothesis. The CRiSP/Alpha model was derived from a mechanistic basis. A basic assumption of the FLUSH transport mortality hypothesis is that conditions experienced by the in-river fish affect survival of both transport and non-transport groups. In contrast, in CRiSP the survival of each group depends on the passage conditions experienced by each group respectively. Through a test of several hypotheses it was concluded that transport fish delayed mortality depends on stress, which can be indexed by descaling measured at transport collection. Evidence is presented to show that the ratio of delayed mortality of transport to non-transport fish is independent of the number of dams non-transported fish pass and the time of arrival of the groups into the estuary. The hypotheses test also indicates that non-transported fish delayed mortality is manifested while fish are in the hydrosystem passage and is adequately described by the TURB4 dam passage hypothesis which accounts for descaling in dam passage.

> It was shown that in the FLUSH delayed mortality hypothesis the delayed survival of transported fish changes in proportion to the non-transported in-river survival. This response is problematic in that changes in the river system below the transport collection site will change the post-hydrosystem survival of the transported fish.

> In terms of transport-to-control (*T/C*) ratios the two models are also different. The FLUSH model assumes *T/C* ratios can be expressed by one variable, survival of non-transported fish. In CRiSP the *T/C* ratios are expressed by two variables; the survival of non-transported fish and the descaling experienced by the transported fish. It is interesting to note that by fixing the average descaling at 5% the two models give virtually identical *T/C* vs. survival curves.

> **References**

> **Anderson, J.J.** 1997. Reconstructing dam passage mortality in years with forebay trash. June 23 1997. In: Review of Dam Passage Routing and Survival, Direct Transport Survival, and Squawfish Removal Effectiveness Estimates for Yearling Chinook Salmon in the Snake/Columbia River System. Draft 5 10/7/97. Reporter, C. Toole- National Marine Fisheries Service.
> **Williams, J. and G. Matthews.** 1995. A review of flow and survival relationships of spring and

summer chinook salmon *Oncorhynchus tshawytscha* from the Snake River Basin. Fishery Bulletin 93:732-740.

Wilson, P., C. Petrosky, H. Shaller, and E. Weber. 1998. Response to J.J. Anderson "Critiques on transport and extra mortality hypotheses", dated December 22 1997. January 8 1998.

Response by Wilson et al The claim that CRiSP hypothesis is derived from first principles, while the FLUSH hypothesis is a consequence of a non-mechanistic functional form appears inconsistent. One point is that the "first order mechanisms" describing the CRiSP hypotheses are constantly changing (as outlined above).   Principles that would seem central to the CRiSP world view at one stage, such as differential hydrosystem mortality of in-river and transported fish having consequences to later life stage survival rate, are ignored in later incarnations.

Because of the complexity of the salmon life-cycle and ecosystem, the STFA hypothesis does not single out one particular mechanism or index among many to predict direct and delayed mortality of transported fish.   Weber et al. (1997) also noted there are many plausible mechanisms likely influencing survival of transported fish, and cited literature in support:

For example, smolts are subjected to the stress of crowding and injury during bypass, collection, holding, and transport. High levels of descaling have been reported (Williams and Matthews 1995; Basham and Garrett 1996). Stress and injury may trigger disease outbreak (e.g., BKD, fungal infection) and delayed mortality. In-river juvenile migrants from the Snake River now enter the estuary later than they did before completion of the hydropower system, whereas transported juveniles may experience a combination of delayed and accelerated migration. Physiological state and time of saltwater entry may be poorly synchronized on both sides of the "biological window" for transported groups. For example, Fagurlund et al. (1995) cite studies of effects of premature saltwater entry (incomplete smoltification) with coho salmon, resulting in high mortality, and, in many of the survivors, a reduction in or cessation of growth. Several of the above potential mechanisms have been identified in the literature (Mundy et al. 1994, Raymond 1988, Williams 1989).

See Weber et al. (1997) for more discussion of mechanisms causing and support for delayed mortality of transported fish.

Survival of transported fish in FLUSH is expressed relative to that of in-river fish, and has been in regional passage models for many years, because until very recently there were no reliable measures of the isolated smolt-to-adult survival of the transported or in-river component of yearly juvenile migrations. The best way to use the limited historical data available (T/Cs) to estimate absolute values of such things as post-Bonneville survival is by using other data (e.g. stock-recruit data) in combination with the T/C data to try to estimate the parameter of interest for one of the components of the migration (e.g. non-transported fish) and so be able to estimate the other with the relative survival data represented by the T/C studies.   One advantage of the STFA T/C model is that it is fit to actual data, on T/Cs and in-river survival, rather than model projections of these quantities (except for 1986 and 1989 T/C studies, where no survival estimates are available).

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End of general comments on Section A.3.3. (Extra Mortality Hypotheses) by Anderson and Response by Wilson et al.

A.3.4 Future Climate

In addition to model components described in Section A.3.2 above, climate variables δ_t (year-effect) in the delta model and the PAPA index in the alpha model are modeled prospectively to reflect different assumptions about future climate. These climate variables are modeled either as a type of autoregressive process or as a combined 18.5 year cyclical/ autoregressive process.

Estimated flow at Astoria was used to investigate post-Bonneville effects of flow. The Astoria flow index in the alpha model showed no significant auto-correlation properties in the exploratory analysis by Hinrichsen and Anderson. A small but significant correlation exists between 1/(Astoria flow) and unregulated water transit time (R=.29). This low level correlation was modeled in BSM by choosing flows from years with above median WTT whenever an above median WTT water year was chosen for a simulated year (and vice versa).

 "Markov" (autoregressive) Climate

Rationale:

There is a significant first-order auto-correlation present in the MLE estimates of δ (the year-effects parameters), as seen below in Figure A.3.4-1. Regression of $\delta(t+1)$ versus $\delta(t)$ has an R-square = 0.271 (significant at p=.0008).
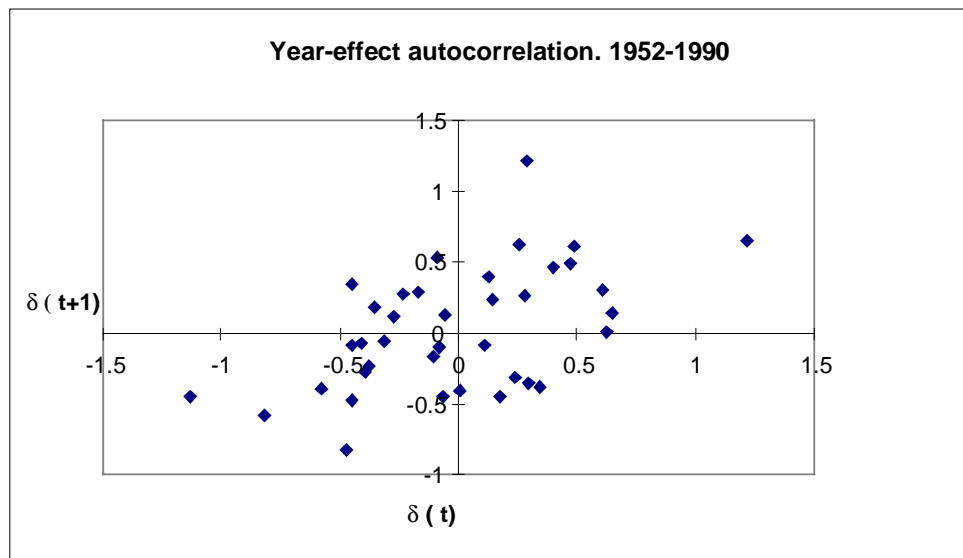
Figure A.3.4-1: MLE estimates of year-effect parameters, the δ's, for 1952 - 1990.

Implementation in Prospective Models:

The autocorrelation apparent in the year-effects parameters was captured in the population projections by a type of Markov process with empirical probability densities. The method implemented in the model consists of the following steps: for each 100 year population projection, a sample of the posterior density of the climate-related variable is made (year-effect parameters for the Delta model, and the PAPA index parameter for the Alpha model). From that sample, MLE estimates are calculated for the multinomial probabilities that characterize the sign of the climate-related variable, such as, $P(x(t)>0 \mid x(t-1)>0)$. Given such a P vector then each simulated climate-related variable follows the multinomial(P) for selection of positive and negative values. The actual positive (negative) values selected are chosen at random from the positive (negative) posterior sample of the variables selected for that particular 100 year projection.

Cyclical Climate

Rationale:

Widespread ecological changes related to interdecadal climate variations in the Pacific have been observed in this century. Dramatic shifts in many marine and terrestrial ecological variables in western North America coincided with changes in the physical environment in the late 1970s (about 1977). The 1977 regime shift is not unique in the climate record nor in the record of North Pacific salmon production. Signatures of an interdecadal climate variability are detectable in many Pacific basin ecological systems (Mantua et al. 1997). Among the salmon species shown to have interdecadal variability were Alaskan sockeye, Alaskan pink salmon, Columbia River spring chinook, and Washington-Oregon-California (WOC) coho. While the climate regime from 1977-present has favored Alaskan sockeye and pink salmon production, it has been associated with decreased production of Columbia River spring chinook and WOC coho.

A regime shift occurs every 25-30 years, whenever the Pacific Decadal oscillation (PDO) makes a polarity switch (positive to negative or visa versa). We simulate the PDO using a square wave with a period of 60 years. In this century, the polarity switches occurred in 1925 (to warm/dry climate), 1947 (to

cold/wet climate), and 1977 (to warm/dry climate). These years correspond to Minobe's (1997) analysis of reconstructed continental surface temperatures, which showed interdecadal oscillations (of period 50-70 years) over the last 3 centuries. The next polarity switch is modeled to occur in 2007 (spring chinook brood year 2005).

Implementation in Prospective Models

The approach taken to model the 18.5 year cyclical/autoregressive process is in essence a Markovian process similar to the process described in Section A.3.4.1, except that the underlying Markov probabilities vary dependent on cycle phase. A sine wave crossing 0 in brood year 1975 for the alpha model (and 1980, best fit, for the delta model) with 18.5 year period was used to identify 2 different phases (when the sine>0 and when sine<0). Simulated values of the PAPA index (or year-effect parameter) were selected in each simulation year according to a Markov process estimated from historical data with the same cycle phase as the simulated year. For example if $\sin[(Y-1980)*2pi/18.5]$ is positive for simulation year Y then a simulated year-effect in the delta model would be selected from historical estimates which occurred during historical years where the sine is positive. Further, the selection process during this positive sine wave would occur based on Markov probabilities estimated from that sub-set of historical data.

Comment on Section A.3.4 (Cyclical climate hypothesis):

> This description of the Cyclical climate hypothesis is confusing – the rationale is for a square wave with 60-year period whereas the description of the implementation describes an 18.5 year cycle.

End of comment on Section A.3.4 (Cyclical climate hypothesis).

A.3.5 Habitat

Rationale

Habitat conditions and natural disturbances or management actions which affect habitat have been widely observed to affect salmonid survival during freshwater rearing (Jones et al. 1997 *Retrospective Report revised chapter 10 PATH FY96 Conclusions Document*). In addition, egg-to-smolt mortality rates which typically exceed 90%, suggest that a significant scope exists for habitat-related changes in freshwater rearing survival. However, relationships between population productivity and habitat condition or actions which affect habitat condition are difficult to quantify. Comparisons of stock-recruitment data for spring chinook salmon generally failed to identify significant correlations between landscape or land use variables and index stock productivity (Paulsen 1997 *Retrospective analyses*). Confounding problems included a lack of adequate measures of habitat quality, incomplete datasets on land use, difficulties in defining appropriate spatial scales, and uncertainties in defining lag times for effects. Thus, while few would disagree that habitat can be a critical limiting factor in freshwater rearing or that changes in land use can affect habitat quality and survival, the effects of any given set of habitat improvement activities on stock productivity cannot be predicted. Prospective analyses of the potential effects of habitat changes on future salmon stock performance were therefore based on plausible changes in stock productivity described by the observed range of variability in stock-recruitment parameters among index populations from habitats of varying condition.

We have focused our attention on the Ricker *a* parameter. Our rationale for this choice is that the stocks of interest are generally accepted to be at levels far below their carrying capacities, based on historical estimates of abundance. This implies that habitat changes, while they may in fact affect both *a* and *b* values, are far more likely to affect the probability of stock survival or recovery through their influence on *a*, which directly affects productivity at low stock sizes. The *a* parameter can be thought of as reflecting the quality of the habitat in the area utilized by the stock for spawning and pre-smolt juvenile rearing. The challenge is thus to judge how changes to habitat might affect average egg-to-smolt survival for the stock, translated into a change in the Ricker *a* parameter.

How much of a change in the Ricker *a* parameter is appropriate to consider for each stock? There is no "right" answer to this question. We have chosen to define a range of no greater than a unit increase or decrease in the *a* value from its current value, whatever that might be. A unit change in *a* is equivalent to an approximately three-fold change in stock productivity (or, in other words, in egg-smolt survival), since productivity (R/S) is approximately equal to e^a at low stock sizes, and $e^1 = 2.7$. We offer three reasons for considering this range in *a* to be plausible:

1. For the Snake River basin stocks, the range in current estimated Ricker *a* values is approximately one;

2. A preliminary analysis of PIT tag recoveries showed an approximately three-fold variation in average recovery rates between releases in wilderness areas and releases in managed areas;

3. Smolt production models developed during the sub-basin planning exercise assumed a three-fold range of smolt density capacities between sites classified as having "fair" habitat, and those having "excellent" habitat.

We are not arguing that this evidence provides a convincing reason for believing that habitat management actions will necessarily increase (or mis-management will decrease) productivity three-fold from the current stock condition. We suspect that making such an absolute judgement would be exceedingly difficult, if not impossible. Instead we use this evidence to suggest that three-fold changes in productivity are plausible. Just *how plausible* such a change is for a given stock and management option will affect the assignment of a probability to such a change, as described below.

Before discussing the assignment of probabilities, however, there is a further caveat. We suspect there is an upper limit on the Ricker *a* value for the stocks from a particular region, defined by the intrinsic productivity of the area as determined by physiography and climate. We have thus constrained the plausible increases in the Ricker *a* value to not exceed the maximum *a* value observed for the up-river stocks. In contrast, we do not believe there is a similar constraint on the down-side; stock productivity can reasonably decline by a factor of three, even if it is relatively low to begin with, provided habitat conditions worsen considerably.

We originally chose to examine two contrasting options for future habitat management. The first option (A) can be described as continued management according to existing habitat management plans in the regions of interest (status quo option). In the ICBEMP reports (Quigley et al. (eds) 1996) three options are presented – our Option A is equivalent to Option 1 in their report. The second option (B) is more akin to Option 2 in the ICBEMP report (active restoration of ecological integrity), although we stress the aquatic habitat component more heavily. We characterize this option by the words, "Make every practical effort to restore and protect anadromous fish habitat". These two options provide contrast in the degree to which habitat protection and restoration will be emphasized – we do not believe an option that reduces emphasis on habitat relative to the status quo is likely for the endangered stocks. The prospective model results in presented in Section 5 consider only Option B, and examine the contrast between this option and the scenario in which no future changes in Ricker *a* values are expected to occur due to habitat management options.

Implementation of Habitat Uncertainty in the Prospective Model

For each stock and habitat option, we judged the probability that the Ricker a value would either (1) increase by up to one unit, but to a value no higher than the observed maximum; (2) remain the same; or (3) decrease by one unit, over the next 48 years (the proposed NMFS recovery standard time frame). We also needed to judge how rapidly the changes would occur, should a change occur at all. This is summarized by specifying the probability that a will have changed to a new value by year 12 (or 24), **given** that it is expected to change by year 48. If the change is likely to occur very slowly (i.e., a gradual reduction of fines in stream substrates following sediment control, or a slow phase-in of a riparian management option) then the probability of the change occurring in twelve years, even if it does occur after 48 years, is very low. On the other hand, if the change is likely to result from a sudden event (more likely for a negative change due to a catastrophic event) that is equally likely to occur anytime during the next 48 years, the probability of a change by year 12 is 0.25 and by year 24 is 0.5, again **given** that the event has occurred by year 48. These values (.25, .5) could also reflect a gradual but steady progression over time towards the 48-year value.

Our judgments of each of these probabilities for each of the index stocks included in the prospective modeling are summarized in Table A3.5.1. In the prospective model, these probabilities were used to determine whether, for a given run, the Ricker a value for the stock being simulated should be modified to reflect a habitat management effect in Year 12, 24 or 48 of the simulation. Again, only Option B was included in the results presented in Section 5.

Using Bear Valley Creek as an example, under Option A we assumed equal probabilities (0.15) of an increase or a decrease in Ricker "a" at 48 years, with the highest probability (0.7) being for "no change" (Table A.3.5-1). If a change was to occur, we judged that an increase in Ricker "a" may occur more slowly than a decrease (see 12 and 24 year probabilities). Under Option B, we judged that an improvement was more likely (0.6) than no change (0.4) or a decrease (0.0).

Table A.3.5.1: Probabilities of future Ricker a values for seven Snake River spring/summer chinook stocks given two alternative scenarios of future habitat management. Prob(no change) means the probability that the a value does not change from its current (prospective simulation year 1) state by year 48 of the simulation. Prob(increase) and Prob(decrease) are interpreted similarly. The "increase" and "decrease" columns list the percent change in a value in the specified direction. Prob(12|increase) is the conditional probability that an increase occurs by simulation year 12, given that it occurs by year 48. The other conditional probabilities – Prob(*year*|*direction*) - have similar interpretations.

Stock	Prob (no change)	Relative Increase in a	Prob (increase)	Prob (12|increase)	Prob (24|increase)	Relative Decrease in a	Prob (decrease)	Prob (12|decrease)	Prob (24|decrease)
Option B									
Imnaha	0.85	12%	0.1	0.1	0.5	29%	0.05	0.25	0.5
Minam	0.85	11%	0.1	0.1	0.5	28%	0.05	0.25	0.5
Bear Valley	0.4	9%	0.6	0.2	0.8	28%	0	0	0
Marsh	0.85	11%	0.15	0.5	0.8	28%	0	0	0
Sulphur	1	6%	0	0	0	27%	0	0	0
Poverty Flats	0.6	14%	0.05	0	0.3	29%	0.35	0.35	0.5
Johnson	0.85	10%	0.07	0.4	0.8	28%	0.08	0.25	0.5
Option A									

Imnaha	0.9	12%	0.05	0.05	0.25	29%	0.05	0.25	0.5
Minam	0.9	11%	0.05	0.05	0.25	28%	0.05	0.25	0.5
Bear Valley	0.7	9%	0.15	0.1	0.4	28%	0.15	0.25	0.5
Marsh	0.85	11%	0.15	0.5	0.8	28%	0	0	0
Sulphur	1	6%	0	0	0	27%	0	0	0
Poverty Flats	0.5	14%	0.05	0	0.3	29%	0.45	0.35	0.5
Johnson	0.85	10%	0	0	0	28%	0.15	0.25	0.5

A.3.6 Hatcheries

Aggregate releases of hatchery fish have increased significantly over the last two decades (Table A.3.6-1). Preliminary work suggests that negative, statistically significant correlations do exist between aggregate hatchery releases and Snake index stock survival. Relationships between subbasin releases and survival of individual index stocks is more ambiguous: sometimes the releases appear to have been beneficial, while for others they were problematic.

However, we did not include these relationships in the preliminary prospective analysis. Although there is an association between increased hatchery releases and decreased survival (see Figure A.3.6-1), interpretation of this association is complex. The releases were intended to mitigate for the effects of the Snake River dams. As such, one would expect that as more Snake River dams came on-line during the late 60's and early 70's (reducing smolt to adult survival of wild fish), this would have been accompanied by more hatchery releases to enhance the fish population. This is indeed what occurred. To the extent that the passage models account for the decrease in smolt-to-adult survival (m in equations found in Section 4.3.3) this should be accounted for in the model which includes aggregate releases. It is obvious, however, that PATH is testing many hypotheses which posit that the direct effects of passage (m, above) do not completely account for the effects of the hydrosystem. While the increase in hatchery releases may well have played a role, we have not devoted enough time to the hatchery analysis to disentangle the effects of hatcheries from other factors, such as climate and delayed transportation effects. This work will be completed prior to the final spring/summer chinook decision analysis.

Figure A.3.6-1: Snake Yearling Chinook Releases vs. Snake "Alpha", Brood Years 1970 – Present (Preliminary)

Table A.3.6-1: Aggregate Releases of yearling Chinook, Snake and Bonneville-McNary,

Brood Years 1970-Present, Millions (Preliminary). This table does not consider steelhead and subyearling chinook releases.

Brood Year	Snake Releases	Bonneville-McNary Releases
70	3.46	3.86
71	4.9	1.01
72	3.16	2.52
73	6.34	1.45
74	7.27	3.30
75	5.01	2.91
76	7.75	2.15

77	6.37	6.34
78	7.35	5.06
79	4.04	5.80
80	3.82	4.86
81	6.71	4.89
82	8.75	3.87
83	10.9	6.65
84	7.22	4.54
85	11.6	5.73
86	10.9	7.14
87	12	7.14
88	13.5	12.41
89	9.71	11.16
90	11.7	5.69

Appendix C: Lower Snake River Feasibility Study
Description of Operational Alternatives

C1. Summary Description

A Group - Lower Snake River Drawdown

Alternative A1(Base Case) – This is the base case as it is today. There is Columbia and Snake River flow augmentation as described in the BiOp.

Alternative A2 – This is the future without drawdown condition. It assumes all fish passage is working with the lower Snake and John Day projects not drawdown. Columbia and Snake River flow augmentation would change to a level which will be identified during the study.

Alternative A3 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels. There is no change in flow augmentation from A1.

Alternative A4 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and no Columbia or Snake River flow augmentation.

Alternative A5 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and no Snake River flow augmentation.

B Group - John Day Drawdown to Natural River

Alternative B1 – This is the alternative which shows the Lower Snake and John Day projects drawn down to natural river levels. There is no change in flow augmentation from A1. Or, A3 with John Day drawn down to natural river levels.

Alternative B2 – This is the alternative which shows the Lower Snake and John Day projects drawn down to natural river levels. There is no Columbia or Snake River flow augmentation. Or, A4 with John Day drawn down to natural river levels.

C Group - John Day Drawdown to Spillway

Alternative C1 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and John Day drawn down to the spillway crest. There is no change in flow augmentation from A1. Or, A3 with John Day drawn down to the spillway crest.

Alternative C2 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and John Day drawn down to the spillway crest. There is no Columbia or Snake River flow augmentation. Or, A4 with John Day drawn down to the spillway crest.

C2 Detailed Description of Alternatives - Comparison of Operating Requirements using Alternative A1 as the Base Case (16/01/98)

Several alternatives are being considered in the Lower Snake River Feasibility Study ranging from current operations to natural river level drawdown on the lower Snake River and John Day pool with varying

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APPENDIX C: Lower Snake River Feasibility Study

amounts of flow augmentation. This paper summarizes the specific operating requirements of the base case alternative, lists the other alternatives and identifies how the requirements for these other alternatives differ in comparison to the base case. For the other alternatives, only requirements that change from the base case are noted.

Alternative A1: – This alternative represents how the Federal system is currently operated under the 1995 Biological Opinions (BO) issued by the National Marine Fisheries Service (NMFS) and the U.S. Fish and Wildlife Service (FWS). It is the base case or "no-action alternative" for this study.

Alternative A1 includes the following operating requirements and provisions (which will be used as the categories of comparison for other alternatives):

Flood Control – Current Upper Rule Curves using forecasted and observed runoff and incorporating a shift of system flood control from Dworshak and Brownlee to Grand Coulee. In addition, the current flood allocation between Mica and Arrow reservoirs.

Initialization of Storage Projects – all projects start the regulation full except for Mica (at July target), Grand Coulee (at 1280 feet), Brownlee (at 2067 feet), Libby (at 2439 feet), Dworshak (at 1520 feet), John Day (at 262.5 feet) and Corra Linn (at 1743.32 feet). Generally, these elevations represent the BO summer draft limits.

Canadian Project Operations – Mica, Duncan and Arrow are operated to the 1997 Assured Operating Plan (AOP) with changes agreed to through the 1997 Detailed Operating Plan (DOP). Arrow will store up to 1 MAF in years when The Dalles volume forecast is less than 90 MAF. The stored water is released from April 16 through June.

Libby Operation – Project is operated September through December to achieve 2411 feet end of December elevation, on minimum flow or for flood control through mid-April, for sturgeon mid-April through July, and up to full powerhouse outflow and to elevation 2439 feet through August in support of McNary flow targets.

Hungry Horse Operation – Project is operated September through December to specific end-of-month elevations (3515 feet by December), on or above Biological Rule Curves through March, on or near flood control through June, and to 3540 feet through August in support of McNary flow targets.

Albeni Falls Operation – Project is operated at 2060 feet in September, at 2055 feet October through April, at 2057 feet in May, and full (2062.5 feet) June through August.

Grand Coulee Operation – Project is operated September through December for power generation, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Dworshak Operation – Project is operated on minimum flow or to flood control all months except April through August when it drafts to 1520 feet in support of Lower Granite flow targets.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF is provided from the upper Snake River. Brownlee is operated to flood control and a maximum of 9 kcfs mid-October through November, operated at 2070 and 2060 feet by end of December and January, respectively, on flood control February through April, operated to 2069 feet in May, attempts to refill in June, drafted to 2069 feet in July, drafted to 2050 and 2048 feet in August 1 and August 2, and operated to 2050 and 2048 feet in September and October.

Lower Snake Project Operations – Projects are operated at Minimum Operating Pool (MOP) April 10 through August for Little Goose, Lower Monumental, and Ice Harbor and through November 15 for Lower Granite.

John Day Pool Elevation – Project is operated at 262.5 feet from mid-April through September.

Peak Efficiency – All lower Columbia and lower Snake projects are operated within 1% of their peak efficiency March through November.

McNary Flow Target – A sliding scale monthly or period flow target from 220 kcfs to 260 kcfs applies from April 20 through June based on January-July runoff forecasts, and a target of 200 kcfs applies in July and August.

Lower Granite Flow Target – A sliding scale monthly or period flow target from 85 kcfs to 100 kcfs applies from April 10 through June 20 based on April-July runoff forecasts, and a sliding scale target of 50 kcfs to 55 kcfs applies from June 21 through August.

Spill – All eight lower river projects provide fish spill during the spring period and non-collector projects provide spill during the summer period. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville, The Dalles and Ice Harbor spill 24 hours a day, remaining projects spill 12 hours per day.

Alternative A2: – This alternative represents Federal system operation without drawdown on the lower Snake River or John Day reservoir, relies on fish transportation as the primary method for fish passage and assumes the current level of development of fish facilities. This alternative eliminates fish spill at fish transportation projects.

All requirements remain the same as in Alternative A1 except for spill.

Spill – Four of the eight lower river projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and Ice Harbor. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville, The Dalles and Ice Harbor spill 24 hours a day, John Day spills 12 hours per day.

Alternative A3: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects are operated within 1% of their peak efficiency March through November.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative A5: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels on a permanent basis. Flow augmentation at BO levels on the Snake River is eliminated. Flow augmentation on the Columbia River, spill, and other requirements remain the same as that provided under the BO and Alternative A1.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated.

Initialization of Storage Projects – Dworshak is initialized at 1600 feet.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects are operated within 1% of their peak efficiency March through November.

Lower Granite Flow Target – The sliding scale monthly or period flow augmentation target is eliminated.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative A6: – This alternative represents Federal system operation in the future without drawdown but reflect operational changes brought about by installation of various fish passage improvements such surface bypass collectors, gas abatement facilities, etc. Flow augmentation is sized to match with the available facilities and appropriate fish passage routes.

[Operating requirements have not yet been determined for this alternative]

Alternative B1: – This alternative represents Federal system operation with the four lower Snake River projects and John Day reservoir drawn down to natural river levels on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A3 except for the addition of John Day drawdown

Flood Control – Flood control remains unchanged because it is assumed that some structure is constructed which would allow filling of the reservoir to the flood control volume currently provided by the reservoir.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Pool Elevation - Project is operated at natural river level year round. Generation is eliminated.

Peak Efficiency - Lower Columbia River projects minus John Day are operated within 1% of their peak efficiency March through November.

Spill - Lower Columbia River projects minus John Day provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative B2: – This alternative represents Federal system operation with the four lower Snake River projects and John Day reservoir drawn down to natural river levels on a permanent basis. Flow augmentation at BO levels from both the Snake River and Columbia River is eliminated. This alternative is identical to Alternative A5 except for the addition of John Day drawdown and elimination of flow augmentation from the Columbia River.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated.

Initialization of Storage Projects – Grand Coulee, Libby and Dworshak are initialized at full pool levels.

Grand Coulee Operation – Project is operated for power generation. The minimum pool elevation for May is 1240 feet. The minimum pool elevation for June through July is 1285 feet. The overall minimum pool elevation year round is 1220 feet to allow for Gifford-Inchelium ferry operation.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated at natural river level year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects, except John Day, are operated within 1% of their peak efficiency March through November.

McNary and Lower Granite Flow Targets – The sliding scale monthly or period flow augmentation targets on both Snake River and Columbia River are eliminated.

Spill – Lower Columbia River projects, except John Day, provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative C1: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels and John Day reservoir to near spillway crest on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A3 except for the addition of John Day drawdown. It is identical to Alternative B1 except for the change in the level of drawdown for John Day reservoir.

Flood Control – Flood control remains unchanged with John Day operated as high as 232 feet to provide current levels of flood control space.

Grand Coulee Operation - Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated from 215 to 220 feet year round. Generation occurs at reduced levels according the lower head.

Peak Efficiency – Lower Columbia River projects including John Day are operated within 1% of their peak efficiency March through November.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period. John Day's spill percentage and TDG cap remains the same as in Alternative A1.

Alternative C2: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels and John Day reservoir to near spillway crest on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A5 except for the addition of John Day drawdown and elimination of flow augmentation from the Columbia River. It is identical to Alternative B2 except for the change in the level of drawdown for John Day reservoir.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated. John Day is operated as high as 232 feet to provide current levels of flood control space.

Initialization of Storage Projects – Grand Coulee, Libby and Dworshak are initialized at full pool levels.

Grand Coulee Operation – Project is operated for power generation. The minimum pool elevation for May is 1240 feet. The minimum pool elevation for June through July is 1285 feet. The overall minimum pool elevation year round is 1220 feet to allow for Gifford-Inchelium ferry operation.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated from 215 to 220 feet year round. Generation occurs at reduced levels according the lower head.

Peak Efficiency – Lower Columbia River projects including John Day are operated within 1% of their peak efficiency March through November.

McNary and Lower Granite Flow Targets – The sliding scale monthly or period flow augmentation targets on both Snake River and Columbia River are eliminated.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period. John Day's spill percentage and TDG cap remains the same as in Alternative A1.

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APPENDIX C: Lower Snake River Feasibility Study

Appendix D: Summary of Spring/Summer Chinook "Jeopardy Standard"

The following text describes the jeopardy standard used in the NMFS 1995 Biological Opinion on Snake River spring-summer chinook. Variations from this approach in the current PATH analysis are minor, but are indicated below in *italics*.

D1. Survival Standard

a. Set threshold levels for each population. BRWG (Biological Requirements Work Group) estimates were used by NMFS for the following stocks:

Population	Number of Spawners Annually
Bear Valley / Elk	300
Imnaha	300
Marsh Creek	150
Minam River	150
Poverty Flats	300
Sulphur Creek	150

Recently, Johnson Creek run reconstructions were completed, and PATH participants agreed to the following threshold.

Johnson Creek	150

b. Using simulation models, project population levels over 24 years into the future and 100 years into the future.

c. Determine likelihood that each population will be above its threshold level over each of the two time periods. This is determined from the cumulative distribution of all simulations encompassing the time period. For example, if 500 simulations each projected population levels for a 100-year period, the resulting distribution would consist of 50,000 values.

d. Express probability in Step c. as a proportion of probability of being above threshold during a historical period in which stocks were believed to be relatively healthy. NMFS did not define this historical period, but accepted model results based on all available years prior to 1976 for the Biological Opinion. Estimation of the historical probability follows the same process described in Step c., except the simulation model is calibrated only to observations during the historical period.

PATH participants agreed to leave out Step d, for three reasons. First, a ratio of probabilities can be misleading. If both the historical survival standard and the future survival standard have a probability of 0.1, then the ratio will come out to 1.0, which gives a misleading impression that the stock is in a good condition under the particular scenario simulated. Second, the ratio measure was conceived during a time when there were two life-cycle models in use, ELCM and SLCM. A ratio reduced differences between these two models. However, all PATH analyses have been conducted using one life-cycle modeling framework, BSM, which uses a consistent set of

assumptions. Finally, the prospective analyses consider many possible combinations of conditions, as outlined in Figure 4.1-1 and Table 4.1-1. A ratio of survival probabilities would therefore need to define historical scenarios for each of the over 5,000 combinations in Figure 4.1-1, and match these with the appropriate perspective aggregate hypothesis.

e. NMFS' jeopardy standard is that a "high percentage" of available populations must have a "high likelihood", relative to the historical probability, of being above the threshold level over each time period. NMFS defined "high percentage" as 80% of available populations. The level of 80% does not neatly transfer into a specific number of stocks in the case of the Snake River, where there are seven index stocks. If five of the seven index stocks meet the jeopardy standard, that constitutes 71%; if six of the seven index stocks meet the standard that constitutes 86% of the stocks. *PATH, therefore, applied the standard that six of the seven stocks should meet the NMFS jeopardy standard. That is, we present the probabilities for the sixth best stock.*

NMFS did not define "high likelihood". *PATH has assumed that 70% be considered an approximation of this standard. Some PATH members have suggested reporting results for a range of probabilities between 60%-95%. Results in Section 5 show the actual frequency distribution of probabilities across all alternative aggregate hypotheses, so that one can easily assess the fraction of cases in which the sixth best stock exceeds higher (or lower) probabilities.*

D2. Recovery Standard

a. Set recovery population level. Relevant population recovery goal in NMFS' "Proposed Recovery Plan" is eight-year geometric mean of annual redd counts equivalent to 60% of the pre-1971 brood-year average redd counts.

Although the NMFS draft recovery level is expressed as redd counts, analyses for the biological opinion converted these to estimates of number of spawners (Table D-2). Values used in previous analyses have changed for some stocks as the run reconstruction procedure has been refined.

Table D-2: Recovery levels used for stocks.

Stock	Recovery Threshold (# spawners)
Imnaha	850
Minam	450
Bear Valley	900
Marsh Creek	450
Sulphur Creek	300
Poverty Flat	850
Johnson Creek	300

b. Using simulation models, project population levels 48 years into the future.

This PATH analysis has also looked at population projections 24 years into the future.

c. Determine likelihood that the eight-year geometric mean of each population will be above its recovery level in the 48th year of a simulation (i.e., geometric mean of years 41-48). This is determined from the cumulative distribution of all simulations. *(This analysis also looked at the probability of recovery based on the geometric mean of years 17 to 24.)*

For example, if 500 simulations each project an eight-year geometric mean population level for the 48th year of the simulation, the resulting distribution would consist of 500 values.

d. Express probability in Step c. as a proportion of probability of being above threshold during a historical period in which stocks were believed to be relatively healthy. NMFS did not define this historical period, but accepted model results based on all available years prior to 1976 for the Biological Opinion. Estimation of the historical probability follows the same process described in Step c., except the simulation model is calibrated only to observations during the historical period. *Step d was not done for the Results in Section 5, for the reasons given under Section D1, part d.*

e. NMFS' jeopardy standard is that a "high percentage" of available populations must have a "moderate to high likelihood" of being above the recovery level within 48 years.

NMFS defined "high percentage" as 80% of available populations.

NMFS did not define "moderate to high likelihood". *PATH has assumed that 50% be considered an approximation of this standard. Actual probabilities are listed in the Results section of this report, so that one can assess the effect of raising or lowering the standard. To assess 80% of available populations, PATH used the sixth highest stock, as outlined above in Section D1, part e.*

D3. Considerations for the Simulations

NMFS concluded that survival/recovery probabilities based on simulation models that include depensatory effects are more reasonable than those from models lacking such effects. However, NMFS did not comment on the proper method of implementing depensation in life-cycle models.

PATH implemented a depensatory function in all simulations, calibrated to existing data. See Deriso (1997) for a description of the function and supporting rationale.

References

Deriso, R.B. 1997. Prospective Analysis of Spring Chinook of the Snake River Basin. 33 pp. In PATH Package #1 for the Scientific Review Panel. June 3, 1997.